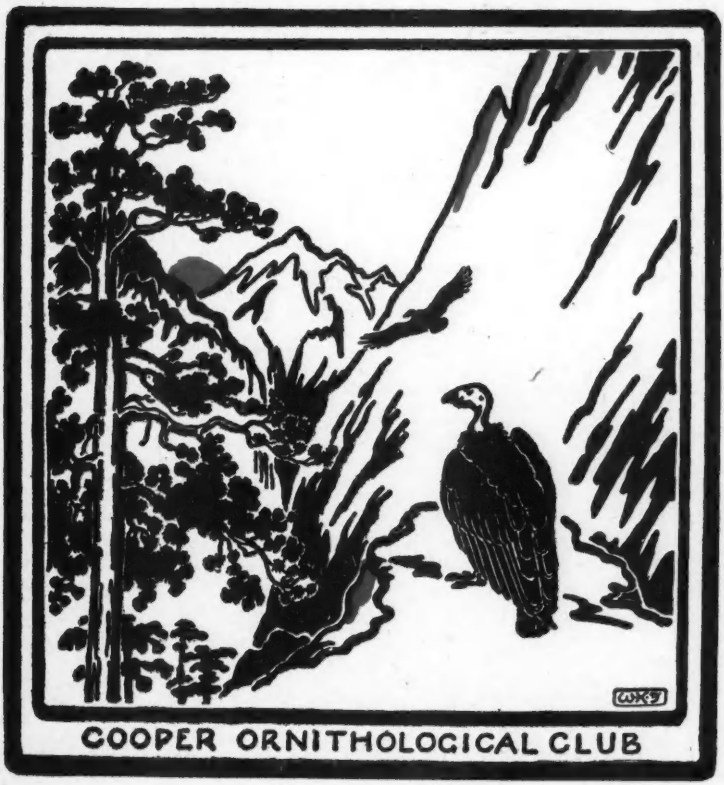
N. Arms

THE CONDOR

A Magazine of Western Ornithology

Volume XXXI July-August, 1929 Number 4



THE CONDOR

A Magazine of Western Ornithology

Published Bi-monthly by the Cooper Ornithological Club

Entered as second-class matter May 15, 1925, at the post-office at Berkeley, California, under Act of Congress of February 28, 1925, Section 412, paragraph 4.

Issued from the Office of THE CONDOR, Museum of Vertebrate Zoology, Berkeley, California.

SUBSCRIPTION RATES

Three Dollars per Year in the United States, payable in advance.
Fifty Cents the single copy.
Three Dollars and Twenty-five Cents per Year in all other countries in the International Postal Union.

COOPER ORNITHOLOGICAL CLUB

Dues are payable in advance on January first for the calendar year: Three Dollars per year for members residing in the United States; Three Dollars and Twenty-five Cents in all other countries. Members whose dues are paid receive THE CONDOR without additional charge.

Send manuscripts for publication to the Editor, J. GRINNELL, Museum of Vertebrate Zoology, University of California, Berkeley, or to the Associate Editor, JEAN M. LINSDALE, same address.

Send dues, subscriptions, orders for back numbers of THE CONDOR and for the PACIFIC COAST AVIFAUNA series to the Business Managers, W. LEE CHAMBERS, Box 123, Eagle Rock, California, or HARRY HARRIS, Box 123, Eagle Rock, California.

Issued July 15, 1929

CONTENTS

PAGE

Nesting of the Laughing Gull in Southern California (with one ill.) ..... *Loye Miller* and *A. J. van Rossem* 141

Roadways as they Affect Bird Life ..... *Jean M. Linsdale* 143

The Whooping Crane Continues to Visit Louisiana ..... *E. W. Nelson* 146

The Function of the Oil-gland (with three ills.) ..... *J. Eugene Law* 148

Notes on Oometry (with one ill.) ..... *Griffing Bancroft* 157

An Untilled Field for a Revised Kind of Research in Zoology ..... *Wm. E. Ritter* 160

A New Cormorant from the Miocene of California (with two ills.) ..... *Loye Miller* 167

On the Subspecific Validity of *Anser gambelli* Hartlaub (with three ills.) ..... *Nagamichi Kuroda* 173

FROM FIELD AND STUDY

Immature Song Sparrow in Full Song ..... *Wm. E. Sherwood* 181

The White-tailed Kite in Orange County, California ..... *John McB. Robertson* 181

Poor-will Noted in San Francisco County, California ..... *Selma Werner* 181

A Record of the Red-breasted Goose in California ..... *A. W. Anthony* 181

A Record of *Tyrannus melancholicus occidentalis* for the State of Washington ..... *A. J. van Rossem* 182

EDITORIAL NOTES AND NEWS 183

MINUTES OF COOPER CLUB MEETINGS 184

THE CONDOR

A Bi-monthly Magazine of
Western Ornithology

Published by the
Cooper Ornithological Club

VOLUME XXXI | JULY-AUGUST, 1929 | NUMBER 4

NESTING OF THE LAUGHING GULL IN SOUTHERN CALIFORNIA[1]

WITH ONE ILLUSTRATION

By LOYE MILLER and A. J. VAN ROSSEM

On June 9, 1928, the writers, accompanied by Mr. R. B. Cowles, visited the Gull-billed Tern colony discovered on Salton Sea by Mr. J. R. Pemberton in 1927. On approaching the tern colony on the largest of the occupied sand islands, a black-headed, dark-backed gull was seen to rise from the center of the colony. It circled about high overhead and finally came directly over us when it was shot at and missed, after which it flew out to sea. Supposing the stranger to have been pilfering terns' eggs, we walked over to the spot from which the bird had arisen to see how much damage had been done, and were amazed to see, on a little knob of concreted sand, a nest constructed of a few small sticks and twigs and containing three gull's eggs.

We waited nearly an hour in the vicinity of the nest without further sight of the bird which had flown seaward after being fired at, and then rowed over to the next island on which was another colony of Gull-billed Terns, as well as some twenty pairs of Caspians. From the center of this second colony, a single gull flew up; it was immediately killed and the identity of these nesting birds was established as *Larus atricilla*, a species not previously reported at any season from the Pacific coast north of the tropics. The nest of this second bird was of identical situation and construction with that of the first and, like it, contained three eggs (see fig. 53). After a wait of nearly an hour at this second nest, the male of the pair appeared, but it kept well out of range, circling us at a hundred yards or more and sometimes flying out of sight. Finally it settled on the nest and by keeping behind sand lumps we were able to come within range.

Returning now to the first island, it was found that one of the pair was on the nest. By this time we were pretty well done in with the heat and the glare from the sand and water, so decided to forego making a census of the Laughing Gulls on the several other islands of the group.

A few days later, on June 22, van Rossem, accompanied by Donald Dickey, again visited the islands, but because of rough water, only the island nearest the shore could be inspected. There were no gulls on this island, but at least two were seen flying about two islands lying farther out to sea, so it is fair to assume

[1] Contribution from the California Institute of Technology.

they were breeding there. Possibly more were present, but the many Caspian Terns flying about made detection of the similar sized gulls a difficult matter, especially as the islands were almost directly in the sun.

Two of the specimens collected on June 9 are now in Mr. Dickey's collection, being nos. 22947 and 22948. The third is in the collection of Dr. Miller. The two sets of three eggs taken are also in the Dickey and Miller collections, respectively.

These Laughing Gulls are undoubtedly members of the great flocks which winter along the Pacific coast of Central America where they were seen by van Rossem in assemblages of many hundreds at various Guatemalan and Salvadorean ports in the winter and spring of 1927. Many species of eastern water birds winter in numbers along the Pacific coast south of the Isthmus of Tehuantepec

Fig. 53. NEST AND EGGS OF THE LAUGHING GULL ON SALTON SEA, IMPERIAL COUNTY, CALIFORNIA, JUNE 9, 1928.

Photograph taken by R. B. Cowles.

and it is not surprising, after all, that now and then birds continue north along the shore line instead of crossing over to the Atlantic side with the great majority of their kind.

We follow Dwight, Ridgway and Wetmore in being unable to recognize a North American form as distinct from the Antillean. The wing of the male of the Salton Sea pair measures 335 mm., which measurement is typical for *"megalopterus"*. The female, however, measures but 295 mm., which is about the minimum recorded by Noble (Bull. Mus. Comp. Zool., 60, no. 10, 1916, p. 368) for *atricilla*.

Pasadena, California, May 17, 1929.

ROADWAYS AS THEY AFFECT BIRD LIFE

By JEAN M. LINSDALE

Roadways have such an important part in the welfare of persons in this country that it is natural to suppose that they should affect also the bird life of the land. It is not necessary to call attention of active observers of birds to the circumstance that many birds are killed by being run over by automobiles. The numerous counts, of birds' bodies seen on roadways, that have been made and published, indicate that this fact has invited concern on the part of many workers.

One trait that characterizes all the essays on this topic that I have come across is that the writers fail to consider more than an exceedingly small portion of the questions involved. The usual inference seems to be that the whole problem consists in establishing the already generally accepted reality that some birds die because of reckless or rapid driving of automobiles over the highways.

In order to arrive at dependable conclusions with respect to such a question as this one it seems desirable to consider the whole situation or at least as many phases of it as possible. Here is a problem for which, it seems to me, counts of dead birds found on small stretches of highway and other such meager collections of facts, alone, do not give a good basis for judgment. The influences are too complicated to be represented fairly by such counts.

For the present purposes it is well to consider only graded dirt roads and paved or hard surfaced highways, thus leaving out of consideration the slightly marked trails over plains country, logging roads through timber, and city streets.

In getting at an understanding of the relations of roadways to birds many sorts of factors might be mentioned as having importance. As a rule, the more highly cultivated a region is the more numerous will be the roadways through it. It is to be pointed out here that conditions out in the cultivated fields are usually not suitable for supporting a large bird population.

Even though every one is familiar with the ways in which conditions accompanying roads favor bird life it may be well now to outline briefly some of these benefits. As regards favorable food supply and feeding places: the roadside tangles on the strips of waste land in the fence rows, made up of various kinds of weeds, vines, shrubs, and trees, contribute huge amounts in the aggregate of food, directly in the form of fruits and seeds and indirectly in the abundant insect life which they support. Roadside ditches, which occur so frequently, often provide feeding situations not found elsewhere in the vicinity. Several bird species regularly use for feeding lookouts the perches on wires or posts of fences and telephone lines which follow roads. These birds may fly into the air for prey or they may watch for movements on the ground. Then the roadbed itself often is a favorable feeding place because of the waste grain or other trash left on it. Bodies of dead animals of all sorts left on the roads make an additional source of food.

For shelter the roadside vegetation favors the presence of birds by acting as a windbreak and by providing cover from the view of predators, even if the tangle does not actually deter the advance of these pursuers. Some birds, for example juncos, regularly roost at night in crevices in road-cuts. The dark bare soil of roads and their banks is often freed of snow sooner in winter than vegetation-covered adjacent ground. Birds seek out such places. In hot regions the small shade of each roadside post is usually taken advantage of by some bird.

Many birds are attracted to roadways for the places for bathing which they find there. Drainage ditches along roads often hold the only readily available

water supply in a vicinity for birds' drinking and bathing. Then many birds come to the roads to wallow and flutter in the dust.

The wires, poles and trees which go along with roads furnish resting perches for large numbers of birds such as hawks, swallows, and goldfinches.

Finally, roadsides supply a considerable variety of nesting sites that may accommodate all together great numbers of birds. These sites are chiefly in the roadside vegetation. Hedges of osage-orange which border so many miles of roads supply an amazingly large number of birds with nesting places. The vegetation of roadside ditches is occupied by nesting birds of several species. A few species of hole-nesting birds are especially well adapted to use cavities in poles and posts such as border most roads. Even the Bullock Oriole has placed a nest on a telephone pole brace. Cavities in cut banks of roads are nested in by Carolina Wrens and other species. Bridges attract many nesting pairs of phoebes, dippers, and swallows.

These examples are sufficient to show that the set of conditions which go along with roads is so favorable that a large bird population can live there. Remember that such a large number of favorable circumstances, ordinarily, is found neither in an equal sized strip of primitive territory nor in a strip across cultivated country as it is now.

The harmful features of roads are chiefly of two sorts: Indirectly, the approach to birds and their living quarters is made so much easier that the reduction in numbers of some conspicuous species may be thus accounted for. Then many birds are run over by automobiles. The published counts of dead birds that have been seen in roads establish that the number killed in the whole country is large—just as a summation of the numbers of persons killed in this way each year is an amazingly large number. A study of these counts and of impressions from seeing the birds in roads indicates that most of the deaths are in a few species and that these are common ones. In nearly every instance the bird species is actually too numerous, judged on the basis of human interests. The most conspicuous losses are in birds which occur in flocks or which have an exceptionally slow get-a-way in starting flight from the road. The most conspicuous example of this type of bird is the Red-headed Woodpecker. However, I am sure that there are a great many more of these woodpeckers in at least some parts of the country now than there were at any time before the settlement by men. There has been even an extension to the westward of the range of this bird in late years, apparently following the courses of highways and railways.

Another point to be taken into account is that most often the birds which are run over are ones which live in or next to the roads. They are not attracted there temporarily. A conspicuous exception to this is the case of those transients which die by flying into telephone wires and similar obstacles, but the total number killed in this way must be small when compared with the whole bird population.

Then, the recent discovery that lost mates in nesting pairs are often quickly replaced helps to minimize the losses during the nesting season. Thus the loss of one parent of each of many breeding pairs might have almost no appreciable influence upon the number of young produced in the habitat. The deaths from this cause being almost entirely of birds out of the nest, the stage affected in the bird's life cycle is not one which ordinarily would be thought of as critical. In other words, losses then do not affect population numbers so directly as do such agencies as ones which destroy nests or broods or which prevent the nesting of the birds. For instance, remove the wooden poles and posts from a stretch of road and there will surely be no birds, of the sorts attracted by these features, to be run over by automobiles on that road.

From one viewpoint the question to be answered is not: How many birds are killed by speeding automobiles? but: How many pairs of birds are prevented from successfully rearing broods of young? Might not the influence actually favor some kinds of birds by removing some of the surplus which could not find suitable breeding places but which, if they lived, would reduce the supply of food available for all?

The conditions in the roadside type of habitat may be compared in many ways with the conditions on a narrow strip of ground such as borders so many streams, especially in the central part of the country. Along the streams also the presence of many birds is favored. However, there as well, conspicuous dangers levy heavy toll of bird life. Predatory birds and mammals are undoubtedly more effective enemies of small birds in streamside thickets than along roadways. Frequently recurring high water destroys the lower nests and even carries away or completely covers the food supply of ground foragers. Yet, in these places the bird species which lose most always recover their usual numbers, at least by the following season.

To sum up, this brief sketch of the environmental factors effective particularly near roadways suggests that the beneficial influences upon the birds there more than offset the harmful influences. A further suggestion is that any analysis of roadway mortality of birds, to be complete, must also take into account as many as possible of these favorable influences effective in the area where the observations were made.

Museum of Vertebrate Zoology, University of California, Berkeley, May 18, 1929.

THE WHOOPING CRANE CONTINUES TO VISIT LOUISIANA

By E. W. NELSON

In February and early March, 1928, the writer spent some time investigating the distribution and habits of the Blue Geese (*Chen caerulescens*) on the coastal marshes of Louisiana. On March 11, with the local game warden, Ulyese Veazey, I rode over some miles of slightly flooded, hard marsh lying between the shore of the Gulf of Mexico and the long oak ridge, some eight miles inland, known as Pecan Island. About two miles from the Gulf, at a distance of perhaps a mile, we saw cloud-like flocks of blue geese, aggregating five or six thousand birds, rising and settling uneasily on their feeding ground, filling the air continuously with their chorus of rather highly pitched notes.

The son of the owner of a small cattle ranch on the coast rode with us and on inquiry as to what part of the marsh the big white cranes frequented, of which he and his father had told us, he pointed to the right and farther inland. After following that direction for nearly a mile I was delighted to see a splendid pair of *Grus americana*, their plumage gleaming white in the bright sunshine as they stood in the midst of a little group of half a dozen cattle.

We took a course that would lead us within about 500 yards to one side of them and as we approached that point they arose with slow and stately wing strokes and moved across the marsh, at an elevation of about 100 feet, for about a mile and, alighting, stood on the alert. After watching them for some time we turned away and left them in peace.

I believe that all the members of the Cooper Club will appreciate the exultant thrill I experienced in seeing these majestic birds in life for the first time since the middle 70's when, as a youngster, I roamed the prairies of the Mississippi Valley. As my eyes followed these birds moving deliberately away a feeling of sadness arose as I realized this was probably my last sight of some of the very few survivors of one of the finest birds native to our fauna but doomed to early extinction. So far as I could learn among the trappers and hunters living in these marshes, from the delta of the Mississippi to the border of Texas, this pair is all that survives of the many Whooping Cranes that once wintered there.

On January 7, 1929, Ulyese Veazey wrote me from Pecan Island that this pair of cranes was again wintering where we had seen them in 1928. He had seen them within two weeks of the time he wrote and he frequently heard their rolling call notes from a distance as he worked his trap-line daily in the marsh.

One disquieting element regarding these birds is that this is the fourth successive winter they have passed on this section of the marsh, always without young. Possibly these birds may be of the same sex, but in any case they fail to rear young, so far as the evidence goes, and when they meet their fate, in the not distant future, the species may disappear forever from this region. Formerly Whooping Cranes wintered commonly south to the marshes at the head of Lake Chapala, Jalisco, on the southern part of the Mexican tableland, but they vanished from there many years ago.

Mr. Veazey, who has spent his life on Pecan Island, informed me that years ago, from 15 to more than 20 Whooping Cranes regularly wintered in the section of marsh where we saw the pair. Ordinarily they were seen singly or in groups of from 2 to 5, but on one occasion he counted 21 gathered in a loose flock, but it soon broke up and the birds scattered as usual. He stated that for the last 15 or 20 years these birds have been steadily decreasing.

My first acquaintance with Whooping Cranes was during the early 70's, when I made a practice of prowling along the several blocks on South Water Street, in Chicago, where dealers in game were located. There, of a spring morning, from 8 to 10 Whooping Cranes might be found hanging. They were usually purchased by large restaurants which specialized in game. Their large size, pure white plumage and black wings made them conspicuous, and they always attracted admiring attention as was shown by the number of people who stopped to look at them.

It was in these places that I first became familiar with blue and other geese, many kinds of ducks and waders. The displays of game were fascinating from the amount and variety of species sent to this great distributing point. One never knew what he might find, as the term "game" was an elastic one in those unregenerate days and might include pileated woodpeckers, flickers, robins, meadowlarks, shore-larks, with occasional hawks, owls and numerous other strange feathered victims.

Washington, D. C., April 8, 1929.

THE FUNCTION OF THE OIL-GLAND[1]

WITH THREE ILLUSTRATIONS

By J. EUGENE LAW

It is with some trepidation that I re-open a discussion of the function of the oil-gland in birds. Of certain fundamentals, the evidence seems to be complete. The oil-gland is what its name implies, a gland of oil. I have seen the gland and I have seen an oil-like substance in the gland. I have applied pressure to the gland and I have observed the oil-like substance exuding at the tip of a more or less well defined nipple on the gland. On many species of birds I have seen the wee tuft of oily feathers which top this nipple. On the other hand, I have seen species of birds in which there is no trace of this oil-gland.

It is granted, then, that most birds possess an oil-gland which appears to be functional. What is its function?

Since time immemorial it has been assumed that this function is the lubrication or anointing of the feathers, usually with the added thought that it keeps the plumage from becoming water-soaked. Around this assumption has developed a body of logical and illogical legend which has been and is today accepted as part of the gospel of ornithology.

Previous Discussion. In 1832, Charles Waterton called the attention of ornithologists to the impracticability of the prevailing theory that the function of the oil-gland was to lubricate the feathers. At once a controversial discussion began in the pages of the Magazine of Natural History, and later extended to the pages of The Zoologist. Smothered under forensic fireworks, the dispute ended in a draw with neither side yielding. Repercussion at intervals in the succeeding years has made no apparent inroads on the mass thought. The oil from the oil-gland continues to lubricate the feathers, as of old.

Or did, until yesterday. Today, I am compelled to believe that Mr. Waterton was right, that the function of the oil-gland is not to lubricate the feathers. And to the reasons he presented for disbelieving this pretty theory that feathers are anointed with oil, reasons just as cogent today and as solid as when he presented them nearly a century ago, I propose to add other reasons for discarding this age-old belief, and to present a new theory which, while perhaps hardly easier to establish as fact, yet seems better to fit observed fact.

In taking the position that the feathers were not consciously lubricated by the bird, Mr. Waterton based his conclusions on certain hypotheses which may be summarized as follows:

1. Much of the structure of the body plumage was such that the slightest contact with oil irreparably terminated its fluffy condition.
2. The obvious preening to which birds devote so much of their time is a necessary process in keeping the vermin population at a minimum.
3. The structure of the beak and of the tongue inhibited their use as an intermediate agent in the process of lubrication.
4. Even though it were possible so to lubricate the body plumage, the plumage of the head and neck, whose texture is indistinguishable from that of the body, could not so be lubricated.

The major points on which the proponents of the theory based their conclusions were:

---

[1] Read before the Fourth Annual Meeting of the Cooper Ornithological Club at Berkeley, California, May 18, 1929.

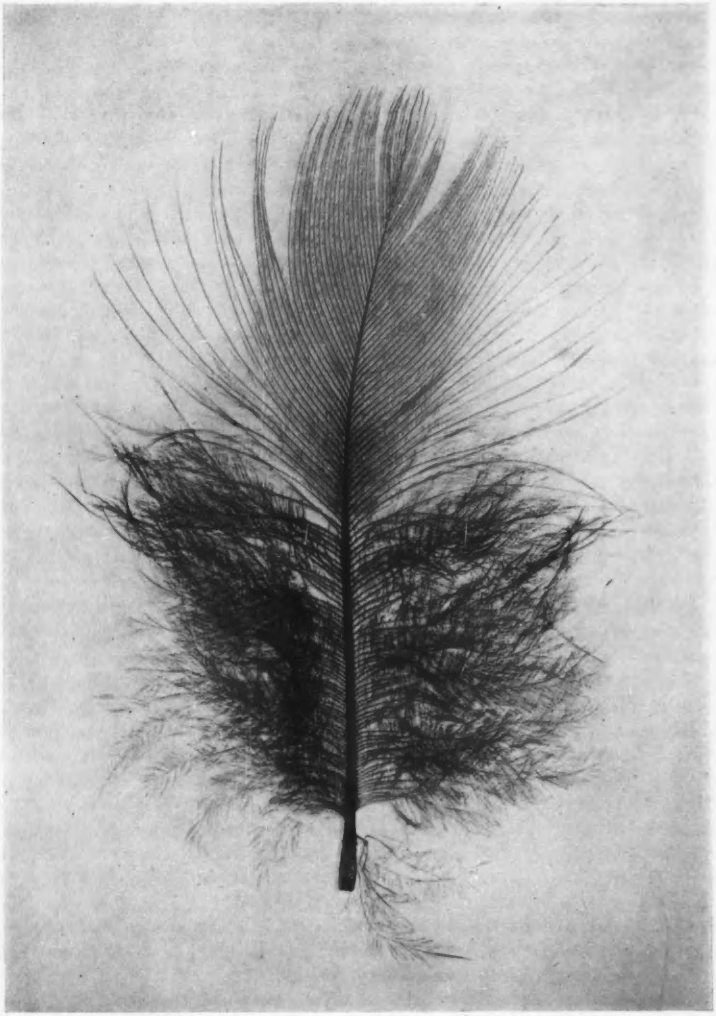


Fig. 54. A SILHOUETTE PHOTOGRAPH OF A FEATHER PLUCKED FROM THE BACK OF A WIDGEON, AT DEATH. ATTENTION IS DIRECTED TO THE PLUMULACEOUS BASAL HALF, THE DELICATE FLUFFINESS OF WHICH WOULD BE DESTROYED BY THE SLIGHTEST TAINT OF OIL. x 3.
Photograph by Joseph Dixon.

1. Observation of frequent actions of the bird in applying its beak to the vicinity of the oil-gland and then to the feathers.

2. In rebuttal of the fourth assertion above: that the bird rubbed its head and neck feathers on the body plumage, and thus lubricated them from the oil that had first been applied to the body feathers.

Mr. Waterton challenged the observations, stating that the birds cited by his opponents as having been observed in the act of oiling their feathers, had their oil-glands so submerged in down-feathers that actual contact of the beak with the gland in a way to convey oil to the beak could not be observed.

Around this controversy, thus briefly outlined, many details of similar nature were woven. Mr. Waterton's assertions had come as such a shock to the orthodox legend of the ornithological hierarchy that they were soon forogtten in a mass of recorded observations not one of which carried the matter to factual conclusion.

Feather Mount Method. My own attention was drawn to the improbability that the oil-gland furnished lubrication for the feathers after I began the method of studying feathers which I outlined in THE CONDOR (XXVII, 1925, p. 123). It excited no particular skepticism when passerine feathers, taped flat on white paper and left there for long periods, produced no oil stain on the paper sheets. But when my experience extended to other groups, including waders and water birds, and no stain appeared on the paper to which their feathers were taped, there seemed to be convincing evidence that feathers do not carry oil on their surfaces.

As the idea developed in my mind, I deliberately sought out freshly killed water birds of the various groups for the purpose of testing this oil theory, and mounted their plucked feathers tightly held against delicately surfaced sheets of paper.

The Case of the Black-vented Shearwater. As a climax, in January, 1929, Mr. George Willett was good enough to shoot and bring to me three Black-vented Shearwaters. I think of birds of the shearwater-petrel group as the oiliest of all. In collection cabinets the feathers on specimens of these forms feel oily, and labels attached to such specimens are invariably saturated with oil. Perhaps these and similar impressions have kept allayed skepticism that might otherwise have developed.

If one raises the feathers on the rump of a shearwater so that the area about the oil-gland is exposed, he finds a broad brush, nearly an inch long, mounted on the tip of the oil-gland. This brush lies flattened in the V-shaped area between the bases of the upper tail coverts. It has the appearance of a miniature tail whose plane is that of the real tail. The oil-gland itself is obscured by dense fluffy down which fairly contacts, as does the downy base of the upper coverts, the very edges of this oil-gland brush or tuft. But, marvel of marvels, the oil-gland tuft is saturated with oil so that the slightest touch leaves oil on one's finger, while the upper tail coverts and the down which surrounds the oil-gland, and the rump feathers which fairly lie on this oil-tuft in natural position, are so free from oil that they may be filed away between folds of delicate onion-skin paper and preserved flat without yielding any oil stain, except for the few shreds which had been in actual contact with the oil-tuft.

Inferential Points. From these observed facts we may draw certain logical conclusions which seem to be of value in solving this oil-gland problem. To begin with, if the oil were of an enamel-like nature, with elements of it quickly volatile so that it dried as a varnish, would we not have this oil-tuft becoming the center of a ball of dried enamel? But there is no ball of enamel. If, on the other hand, the oil were of such a nature that, once applied, it would creep over the surface

of the feathers to a uniform consistency, would we not find it creeping from the oil-saturated tuft of the oil-gland of this Black-vented Shearwater to and through and over all the adjacent feathers? That the oil will creep over feathers seems certain, since the large oil-tuft of the shearwater is uniformly besmeared with it. But the oil does not creep over the feathers which surround the oil-tuft, and it is absent from all the other body feathers.

Right here, it seems to me, Nature herself takes a hand in answering the question of whether or not she wants oil on the body feathers, for in many birds whose oil-glands bear tufts, Nature seems to have gone to considerable pains to provide special skeletonized feathers which in some way insulate the oily tufts from the surrounding plumage. Why these skeletonized feathers not only do not convey oil to the feathers which overlie them but do not themselves receive oil, I have not determined.

Experiments. When we subject the feathers of a bird's body to experiments which should reveal the presence of oil, its absence becomes apparent. Pluck feathers from any part of the body of a swan or shearwater or robin, freshly killed or alive, place these feathers in a folded sheet of delicately surfaced white paper, and run a hot iron over them. If oil were present, oil spots should appear on the paper. But the paper will remain unstained, even until scorched by the iron. Again, place another such pluck of feathers in a clean test-tube filled with distilled water and boil the water over a flame. If oil were present it would rise to the surface of the water, and when cool would leave a smear on the wall of the tube. But I have been unable to detect any smear when feathers were so treated.

Waterproofness of Plumage. Rather obviously, one function of feathers is protection of the bird's body from rain. That feathers seemed to provide this protection probably gave rise to the belief that they were anointed. As we ponder on the thorough way in which plumage sheds water, we can but marvel at the peculiar fitness of the feather covering to serve that purpose without the assistance of oil. Several factors are present which tend to produce waterproofness and to prevent the saturation that would follow capillary attraction. Mode of attachment, structure, and shape of the feather, all contribute in this protective function. Enumerated, these factors which combine to produce waterproofness include:

1. The arrangement of the feathers in the pterylae. This is such that each feather overlaps the adjacent halves of two other feathers: a system of imbrication consistently followed by the man who shingles your roof.

2. The curvature of each contour feather. So convexed are their exposed surfaces that, except when raised nearly to a vertical position, the tips of the feathers curve over and engage the surfaces of the feathers under them. This feature is particularly marked in the birds which spend their time in the water, such as swans, ducks, shearwaters, and the like.

3. The cortex or hardened surface of every element of the feather. This is so glazed that fluids will not readily penetrate its unfractured surface. In other words, the structure of the cortex is, for all practical purposes, non-absorbent.

4. The crisscrossed barbules, subtly hooked together. The closeness and extreme minuteness of the strands of the pennaceous feather fabric are such that, being non-absorbent, the surface tension of water is not broken, except under pressure. By pressure I mean driving rain and contact with wet shrubbery.

5. The resilience or spring of the plumage mass. This, and the fact that all parts of the bird's body are either convex or slanting, tend to make drops of water bounce or glance off the plumage. If droplets collect, as they do in a mist, frequent shakes dislodge them.

6. The warm air-blanket which the plumage retains about the body. This probably adds to the inhospitability of the feather surface toward water.

Flatten a dry body feather on a table and let a drop of water fall on it. Note that little, if any, of the water penetrates the fabric of the feather. Each drop, or if the drop is shattered, each droplet, stands on top of the feather, a spherical bead,

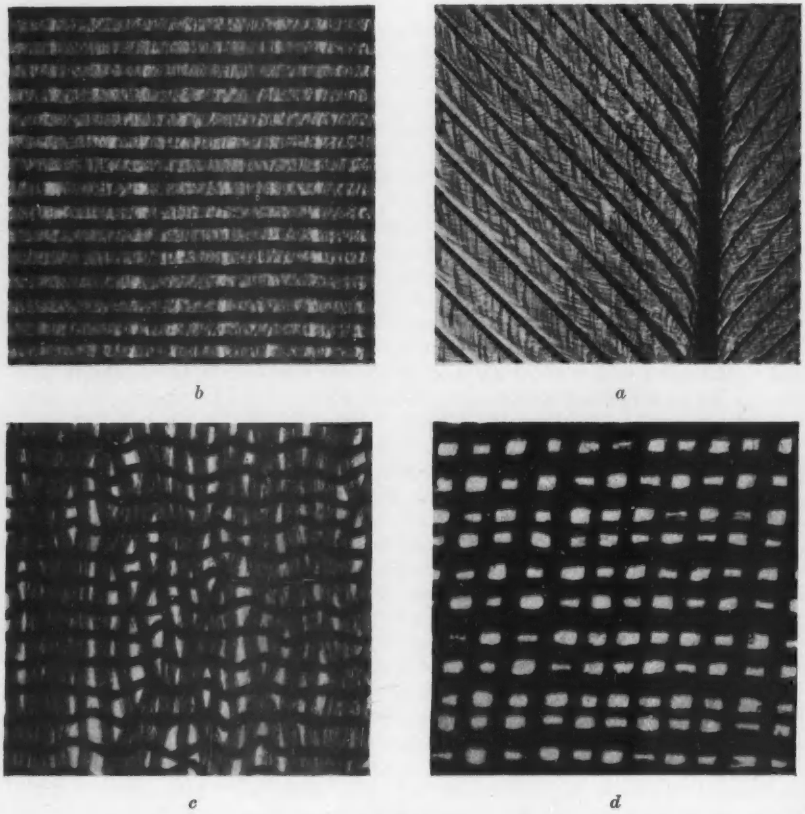
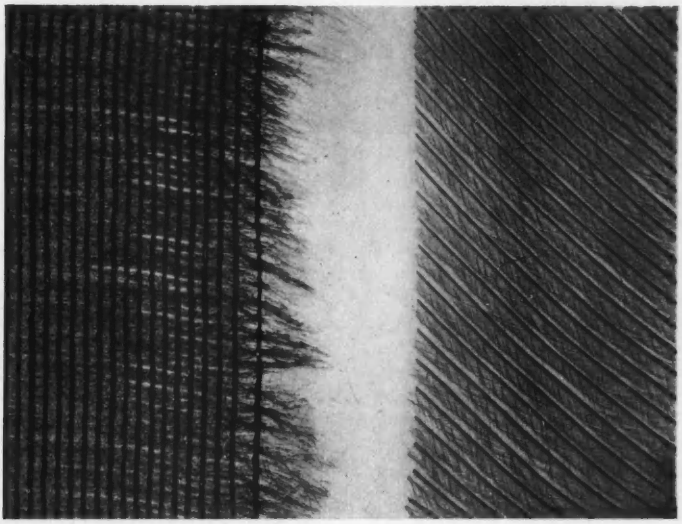


Fig. 55. A MICROPHOTOGRAPHIC COMPARISON WITH IDENTICAL MAGNIFICATIONS OF THE FABRICS OF (a) THE PENNACEOUS WEB OF A FEATHER FROM THE BREAST OF A BLACK SWAN, (b) THE PUSSY WILLOW SILK OF COMMERCE, (c) CREPE DE CHINE, AND (d) HANDKERCHIEF LINEN. THE FABRIC PRODUCED BY THE CRISSCROSSED BARBULES OF THE FEATHER IS THE FINEST OF THEM ALL. x 15.
Photograph by Claude S. Turner.

its surface tension unbroken by the feather elements. A flick of the feather, and the drop rolls off intact. One may first boil the feather if he chooses. The reaction toward water will be the same. If he holds the feather up by its calamus, a position which more nearly approximates the natural position on the bird, a drop of water which strikes it will not tarry on its curved surface.

These factors, likewise, contribute in making it possible for a bird to rest or move in water without becoming water-logged. Not only does the weight of the bird's body tend to seal up the feather mass, by compressing the convexed feathers against one another, but the mesh of each individual feather, on being flattened out, must become materially tighter. This feather mesh thus compressed is so smooth and non-absorbent that the surface tension of the water in which the bird swims is not broken. Nature has accomplished this smoothness by producing what we call "pennaceous feathers." Birds whose plumage lacks the glazing which pennaceous feathers produce become soaked by rain. Van Tyne noted this in the toucan.



a *b*

Fig. 56. SIMULTANEOUS MICROPHOTOGRAPHIC COMPARISON OF (a) THE FIBERS OF PUSSY WILLOW SILK WITH (b) THE FIBERS (BARBULES) WHICH FORM THE PENNACEOUS WEB OF A FEATHER FROM THE BREAST OF A SNOW GOOSE. THE LATTER HAS BEEN CLIPPED TO A STRAIGHT EDGE. x 9.

Photograph by Claude S. Turner

The Function of the Oil-Gland. Now, as to the actual function of the oil-gland. Not long since, I had the pleasure of handling a heron, freshly killed. In contemplating the powder-down tracts which made an oily smear when a glass slide was touched to them, it occurred to me that each of these tracts was in a position which permitted the bird readily to lay its beak against the tract. If, for instance, it turned its head so that the tomia of its beak engaged the oil-gland, the distal portions of its beak would lie against the powder-down tract on its rump. The sort of curves that a heron's neck is wont to take would bring the beak into close contact with the powder-down tracts of the breast. It might even draw its beak through the tract where the leg joins the body. And here, of a sudden,

came the thought with regard to the function of the oil-gland. Could it be a lubricant for the beak?

Physiologically the tissues of a bird's beak may be likened to those of our finger-nails. At its surface the beak sheath consists, in most birds, of dry lifeless cornified tissue, the cells of which are deprived of metabolic assistance. We all know that our finger-nails show direct response to oil, water, and dry heat. Dry finger-nails become brittle and are easily cracked; nails kept oily are pliable. Now, in the economy of the bird the beak is subjected to hard use. Herons, hawks, parrots, even many passerine birds do not spare their beaks. The beak of a water bird, frequently submerged, is now wet, now dry: a most disintegrating thing for any kind of material. To keep the beak effective, then, it would seem that an external lubricant is necessary.

Does it not seem natural that Nature should supply an oil-gland, with the oil of which the beak can be lubricated and prevented from becoming brittle? For this purpose, where in the economy of the bird could a gland supplying this element be better placed than at the base of the tail? Located here, from one side or the other, with a little twisting of the head, the entire beak can be brought into conact with the oil-gland. What more natural than that the oil-gland, to serve such purpose, should be furnished with a delicate brush of specialized feathers? And finally, what more natural than that a bird should polish the oil into its beak on the ready-made polisher which its plumage affords?

In application, this function of the oil-gland can be reconciled to any type of beak, from that of a pelican to that of a hummingbird and from that of a raptor to that of a shearwater. Can we say as much for the older theory that a bird anoints its feathers? One can but be amused at the thought of a pelican, or a skimmer, or a nighthawk, or a raptor, carefully transferring oil from its oil-gland to its feathers. Nor can one believe that the marvelous fabric displayed by a feather from the back of a hummingbird or of a Violet-green Swallow can have the faintest taint of oil on it, or that the down of an eagle (perhaps the most fluffy of all down) has the slightest trace of external oil. Then there is the dainty velvet of the owl's wing. No oil there, we may be sure.

The Bloom on Feathers. In presenting this paper and supporting the theories I have advanced, I am not unmindful of the fact that the down of certain birds and the feathers of certain birds have a bloom which leaves a delicately traced design when pressed between plates of glass, but I am unwilling to believe that this bloom has anything to do with the content of the oil-gland or with oil. In fact, the slightest taint of oil must destroy this fluffy bloom.

Birds without an Oil-Gland. The species of birds which do not possess an oil-gland may be degenerate mutants headed for extinction (which I seriously doubt), or they may have acquired dependence on a type of food, the oily content of which has relieved the oil-gland of its function of lubricating the beak. Thus their oil-gland may have disappeared through disuse. In some, at least, of these birds which have no oil-gland, certain individual feathers in the body plumage are reported to have acquired a specialized condition approaching that of the powder-down of herons. I have examined some glandless parrots in the flesh, but have failed to detect any feathers which were functional for oiling purposes. Moreover, these glandless parrots are clothed in a fluffy immaculate down.

Conclusion. But this is beside the question. I have merely endeavored to substantiate Mr. Waterton's claim that the function of the oil-gland is not the lubrication of the feathers. To his cogent reasons for disbelieving the orthodox

theory, I have added experimental data which indicate that oil is absent from the contour feathers of birds, and analytical data which indicate that the plumage of a bird furnishes a waterproof covering without the assistance of oil. And finally, I have suggested, as a function of the oil-gland, the lubrication of the beak.

BIBLIOGRAPHY

The bibliography which follows lists some of the articles which discuss in detail the oil-gland or its function, or which bear on the controversies which arose about them. Titles which merely mention the presence or absence of the gland have been omitted. No claim is made to completeness. I am indebted to Mr. Harold L. Leupp, Librarian of the University of California, for valuable assistance in adding titles to this list, and to Mr. Donald R. Dickey for the free use of his splendid ornithological library.

Audubon, J. J.
1829. Notes on the Bird of Washington (Falco Washingtonia), or Great American Sea Eagle. Mag. Nat. Hist., 1, p. 119.

Barker, W. G.
1833. On birds lubricating their plumage from an oil gland. Mag. Nat. Hist., 6, p. 162.

Beddard, F. E.
1898. [Oil-gland.] In "The structure and classification of birds.", pp. 18-19. London, New York and Bombay: Longmans, Green, and Co.

Bury, C. A.
1844a. Notes on the birds of the Isle of Wight. Zool., 2, p. 522.
1844b. Remarks on Mr. Waterton's essay of the oil-gland. Zool., 2, pp. 751-755.

Clitheroe, T. C.
1833. On birds dressing their feathers with oil from a gland. Mag. Nat. Hist., 6, pp. 159-160.

Coues, E.
1872. Feather Oil-Gland. Key to North American Birds, p. 5. Salem: Naturalist's Agency.

Daniell, H.
1846. The oil-gland. Zool., 4, pp. 1250-1251.

Gadow, H.
1896. Oil-gland. In Newton's "A dictionary of birds", pp. 653-654. London: Black.

Henslow, H.
1832. On birds oiling their plumage. Mag. Nat. Hist., 5, p. 588.
1833. On birds lubricating their plumage from an oil gland. Mag. Nat. Hist., 6, pp. 277-278.

Hussey, A.
1844. Note on the use of oil from glands for the purpose of lubricating the plumage of birds. Zool., 2, pp. 648-649.
1860. What is the use of the oil-gland at the base of the tail of birds? Zool., 18, pp. 7049-7050.

Matthews, H. S. R.
1861. Oil-gland in birds. Zool., 19, p. 7439.

Morris, F. O.
1835. Notices of the affinities, habits, and certain localities of the dipper. Mag. Nat. Hist., 8, p. 375.
1836a. On birds dressing their feathers with matter secreted from a gland. Mag. Nat. Hist., 8, p. 637.
1836b. Touching the question whether birds oil their plumage, or not. Mag. Nat. Hist., 9, pp. 159-164.
1836c. The question of the office of the gland upon the rump of birds. Mag. Nat. Hist., 9, pp. 269-271, 434-437.
1836d. The late discussion between Mr. Waterton and the Rev. F. O. Morris. Mag. Nat. Hist., 9, p. 610.

Salmon, J. D.
1836. The controversy current between Mr. Waterton and Mr. Morris. Mag. Nat. Hist., 9, p. 492.

Schuez, C.
1927. Illustrierte Zeitung (Leipsic). Noticed in Literary Digest, Nov. 19, 1927.

Stejneger, L.
1888. In "The Riverside Natural History", vol. 4 (Birds), p. 9. Boston and New York: Houghton, Mifflin and Company.

Waterton, C.
1832. On birds using oil from their glands "for the purpose of lubricating the surface of their plumage". Mag. Nat. Hist., 5, pp. 412-413.
1833. The gland on the rump of birds. Mag. Nat. Hist., 6, pp. 274-277.
1835. The dipper. Mag. Nat. Hist., 8, pp. 514-516.
1836a. On the robin and dipper, and rumped and rumpless birds. Mag. Nat. Hist., 9, pp. 158-159.
1836b. The question of the office of the gland upon the rump of birds. Mag. Nat. Hist., 9, pp. 266-269, 324-326.
1836c. Mr. Morris' remarks on Mr. Waterton. Mag. Nat. Hist., 9, p. 610.
1844. Essays on natural history, chiefly ornithology. Second series. London: Longman, Brown, Green and Longmans.
1860. The oil-gland in birds. Zool., 18, pp. 7103-7104.

Westwood, J. O.
1833. On birds using oil from glands. Mag. Nat. Hist., 6, pp. 160-161.

Wetmore, A.
1920. The function of powder downs in herons. Condor, 22, pp. 167-170.

Altadena, California, April 17, 1929.

NOTES ON OOMETRY

WITH ONE ILLUSTRATION

By GRIFFING BANCROFT

The purpose of this paper is to show how average egg sizes may be measured and how the resulting figures may be interpreted. Obviously if, in a restricted area, all the eggs of a given race of birds were to be measured an average size for that district could be determined. This absolute average, even though impossible of getting, can be approximated closely. How many eggs must be measured to do so depends on how greatly those of one set vary from those of another and also on what degree of accuracy is desired. In general I find that about seventy-five eggs give a figure correct to one-tenth of a millimeter, but often a smaller number is sufficient. If two places of decimals are carried the hundredths of a millimeter serves only to qualify the tenth.

There is individual variation in the sizes of eggs. Those of one set of Song Sparrow's eggs, for instance, may be twice as large as those of another. So we must apply mathematical laws of chance and make use of a large enough number of specimens to offset accidental selection. That will have been accomplished when an increase in the number already measured will not materially change the average sizes. I give, as an example, a table of measurements of Song Sparrow's eggs from El Rosario, Lower California, Mexico. The selection was made partly because this is the largest series I have measured and partly because it is the least satisfactory. The individual variations of the eggs of these sparrows are the greatest within my experience. Therefore we can safely conclude that almost any other series of measurements would be well within the range of error here obtained. In making the table I measured twenty-four eggs at a time so as to have a check. I have copied the figures just as I jotted them down in my notebook:

24 eggs averaged 20.35 by 15.19
48 eggs averaged 19.94 by 15.08
72 eggs averaged 20.04 by 15.09
96 eggs averaged 19.98 by 15.10
120 eggs averaged 19.97 by 15.09
144 eggs averaged 20.09 by 15.12
168 eggs averaged 20.10 by 15.15
192 eggs averaged 20.06 by 15.15
216 eggs averaged 20.03 by 15.14
240 eggs averaged 20.04 by 15.14
250 eggs averaged 20.06 by 15.15

I carried this series to unusual length in order to be sure of the closest tenth of a millimeter. It is quite obvious that if I record the average of these eggs as 20.05 by 15.15, I have not been more than one-twelfth of a millimeter wrong at any time after the first three measurements. So even with this knotty problem I can safely draw deductions that are based on accuracy to within a tenth of a millimeter.

Such accuracy is necessary because the value of the work lies in comparative rather than in actual dimensions. It is perhaps of not great moment to science whether the average width of Song Sparrows' eggs is 15.1 or 15.6 millimeters. But it is of great importance to know whether those of one district are or are not greater than those of another, that is, if we really do know. Constant differences are pregnant with meaning even though they be but fractions of a millimeter.

To make the necessary measurements there is needed an instrument of precision. One that should prove satisfactory is illustrated in the accompanying drawing (fig. 57). It is of wood with a steel rod sunk flush in one side. For its proper use there is first required a determination of the zero point. No matter with how much care the meter rod be put in place or perfect right angles be attempted a small error is to be expected when one deals with tenths of millimeters. The zero point can be determined by measuring twenty small eggs at one time. If the same eggs are remeasured in two batches of ten each, and in four of five each, simple mathematics will give the constant error.

In measuring the length of eggs they should be so placed in the trough that small ends are always in contact with small ends, and, conversely, large ends with large ends. If the eggs are held parallel to the trough and perfect contacts are established the sum of the lengths of any number of eggs, not exceeding a half a

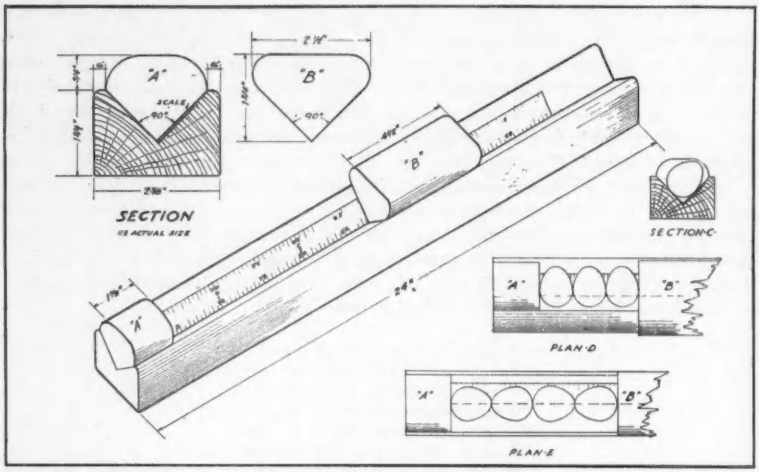


Fig. 57. OOMETER DESIGNED AND USED BY THE AUTHOR OF THE ACCOMPANYING ARTICLE.

meter in the aggregate, may be determined in one reading. This reading should be made to the nearest tenth of a millimeter and the net result, when divided by any considerable number of eggs, is accurate beyond the requirements of this work. When proper care is used this method should eliminate the personal equation and a number of different workers should be able to obtain identical results. The use of calipers or of any other devices on the market shows far too much individual variation.

To measure the width of eggs they simply have to be placed upright on their large ends and allowed to lean against one side of the trough in contact with each other. The sliding plug is shoved against them and an accuracy is obtained that cannot be approached by any other process. It is decidedly greater with minor than with the major axes.

If minimum and maximum figures are desired they can be obtained separately. Personally I consider them meaningless. I have runts and abnormally large eggs

in nearly every extensive series in my collection, but I fail to see the value of keeping any record of them. Of course I disregard the obvious runts, but where sports end and natural but extreme sizes begin, is, at best, a matter of opinion.

The value of oometry lies in the determination of delicate variations in the sizes of the eggs of birds of the same species. Comparative skin measurements often show little or nothing regarding the actual size of the living specimen. One subspecies may have shorter wings, tail, tarsi, or claws than another and still be as large or larger. Furthermore, comparative skin measurements are not susceptible to great accuracy. Their value is weakened by personal factors, and they cannot be or are not used in sufficient numbers.

The proper interpretation of egg measurements follows as a corollary upon their determination. For instance, under ordinary circumstances little value can be attached to tables which show longer eggs in one group and wider eggs in another. That would mean an average difference in shape, a difference which exists freely enough between species but has not as yet been found in subspecies to a sufficient degree of certainty. The student can readily tell when his figures have become stable. When that has been accomplished he can make comparisons and analyses founded on definitely scientific data. I do not feel that I am exaggerating by claiming that the comparative factors disclosed rank equally in importance with chromatic and skeletal differentiations.

As an illustration see my article in THE CONDOR (XXIX, 1927, p. 31). There I published the following figures on the Western Gull, *Larus occidentalis*. From Todos Santos and Los Coronados Islands, 50 eggs average 2.78 x 1.95 inches; from Scammons Lagoon, 50 eggs average 2.89 x 1.98 inches; from San Luis Island, 30 eggs average 2.89 x 1.99 inches.

Let us see what can safely be deduced from figures. It is clear that the eggs of *livens* are not only much larger but are also more elongated than those of *wymani*. I argued at the time of publication that the dimensions prove that *Larus occidentalis livens* is really *Larus livens*. If there were intergradation it would appear in the figures, for yellow legs and pink legs do not account for the table above. In 1928, I found downy young of the Western Gull in the Gulf of California. Their ground color is almost white, as contrasted with the rich brown of baby *wymani*. So the inclusion of both forms in one species is not tenable. It is also clear that the birds of Scammons, though they have pink legs, are also *livens*, and that they and the birds of the Gulf are subspecies of each other. Is there not justification for naming a new gull, with characteristics of larger eggs than in *L. o. wymani* and with a range of at least the west-central shores of Vizcaino Bay? The birds differ from *L. o. livens* in having pink legs instead of yellow ones.

San Diego, California, April 1, 1929.

AN UNTILLED FIELD FOR A REVISED KIND OF RESEARCH IN ZOOLOGY[1]

By WM. E. RITTER

You notice my title is double-barrelled. The suggestion of a revised kind of research is one barrel, and of an untilled field of research is the other.

The revised kind of research has reference to research for answering the questions, In what way and how well do animals use their heads toward solving the problems by which they are always confronted under natural conditions? The intimation that these questions constitute an untilled field for research may seem a bit cryptic or even dubious. But I hope a brief presentation of a few facts and principles will convince you that such is not the case.

Wishing to be as matter-of-fact as possible, I am going to illustrate my points as far as I can by my studies on the California Woodpecker (*Balanosphyra formicivora bairdi*). What the king-pin in the life career of this bird is, no member of the Cooper Club needs to be told: *Acorn storing!* That is the way the phenomenon is pretty sure to drop into one's mind the moment he hears the bird's name. But do not fail to note that the storing business is only an aspect in the utilization by the birds of one of California's most widely distributed, abundant, and nutritious native food crops.

The idea of an animal's "using its head" is meant seriously in this discussion: None of the levity about it that goes with the parental reproof of the thoughtless child that if he would only "use his head" he would not make such blunders. The California Woodpecker's head is for us literally what you have if you collect this part of a specimen by decapitation, plus what the part was while it was still part of the living bird. That means, of course, a great many sub-parts—too many in fact to mention. But a few of these are of very special importance. For instance, the beak and mouth, the eyes, the ears, some of the neck muscles, and particularly the brain. Now, certain it is that a California Woodpecker uses its head quite differently toward solving its food problem from the way a Red-breasted Sapsucker, for example, or a Flicker, uses its head for the same purpose.

My leading question as applied to the California Woodpecker boils down, then, to this: What, if any, advantage, does this species get in the struggle with its food problem over woodpeckers which use their heads differently on the same problem? Do any of you doubt that this is a legitimate question to ask? But further, have you given it attention enough to be prepared with more than an exceedingly general, largely hypothetical answer? If you have, and have published your conclusions, you will do me a great favor by giving me the bibliographic references, for I have somehow missed them in my search of the literature on woodpeckers. Now I suspect you would be inclined to say that although the question may be legitimate just as a question, it is not fruitful as an actual research problem.

The main aim of this paper is exactly to contend that it is an exceedingly important problem, and is wide open to scientific investigation. Hence the suggestion of an "untilled field" of research. Although I have spoken and written on this subject rather frequently during the last decade or more, not until the last year have I gone into it systematically and persistently enough really to clarify my own thought—to say nothing of the thought of others—on the matter. While from

[1] A paper read at the annual meeting of the Cooper Ornithological Club, held in Berkeley, May 18, 1929.

personal observation and from the observations of many others, some attention has been given to a wide range of animal species, my one really concentrated effort has been on the California Woodpecker.

The answer (in part) to my question is: Several advantages accrue to the California Woodpecker as compared with its nearest of kin, from the unique way it uses its head in dealing with its food problem; (1) its survival ability is enhanced as registered in its population numbers; (2) its ecologic range is somewhat extended; and (3), and probably most significant of all, its domestic and social life is benefited.

Details of the evidence on which these conclusions rest cannot be even touched in this brief paper. As for (1) and (2), details enough, I hope, to turn the trick of proof are given in an extensive paper soon to be published in *The Quarterly Review of Biology*. So I must expect you merely to take or not take, as you choose, my word, pending a chance to read this paper. Although considerable evidence is in hand bearing on (3), that is, the domestic-social life of the species, this is dealt with only incidentally in the paper, and much more observational work is necessary on some of the points.

Now some persons may query in this wise: Granted that this particular bird, the California Woodpecker, uses its head in such a way as to make investigable problems of the kind you indicate, does it follow that other birds, and animals generally, do the same thing? Is it not true that the very exceptional character of this bird's way of living makes it present problems which have no counterpart with most species? Not so, I am convinced. All species—or at least all genera—of birds and mammals at any rate, present the same class of problems only in varying degrees of conspicuousness. Really it is the old familiar question of food habits, put, however, with more particularity than it usually is.

And this brings me to a few reflections on the legitimate query as to how it has happened—if it really has happened—that an important section of the phenomena of animal life should have remained even relatively uncultivated so far as research is concerned. At first thought it may seen surprising, if not incredible, that such a thing should have happened. But on second thought it will, I believe, be seen to be neither incredible nor surprising. The phenomena we are considering are a sort of no man's land between biology and psychology, as these sciences are academically conceived. The persistence with which these two provinces of knowledge of living nature have tried to stay apart, often rather contemptuous of each other, has been a serious spiritual disease of civilized mankind, and cries out to be cured.

Returning to woodpeckers, note that the leading question asked is one the answer to which, if it can be scientifically answered at all, must be answered through field research almost entirely. By its very nature the problem would no longer be that problem were the birds removed from their natural conditions and placed under experimental control.

It would seem then that animal ecology, a modern differentiation in the science of living nature, would be the subdivision of zoology into which our problem would fall. So let us look at ecology with reference to this matter. Were we to take at its face value almost any definition of ecology, we might suppose it would be the natural place for our problem. For instance "ecology is that branch of general physiology which deals with the organism as a whole, with its general life processes as distinguished from the more special physiology of organs" (Shelford, *Animal Communities in Temperate America*, 1913, p. 32). This is certainly broad

enough, and by implication specific enough to include such a question as ours. Yet I find scarcely a hint of any such question in this important book.

Nor does this type of question fare much, if any, better in the very latest ecological writings. "Ecology is a new name for a very old subject. It simply means scientific natural history" (Elton, *Animal Ecology,* 1927, p. 1). Devoted to the ecology of animals, as is the excellent book thus drawn upon, one might suppose questions like ours would be appropriate to it. Yet apparently the author of the book does not think so. At any rate nothing in its pages indicates that he does.

On the whole it appears that by a sort of tacit agreement, possibly on grounds of convenience, phenomena of the kind we are considering are excluded from ecology. But I suspect the exclusion implies something much more fundamental than mere convenience. It is deeply and firmly rooted, I believe, in biological theory. Look at it a moment from the standpoint of numbers of individuals—from the standpoint of the population problem.

Since Darwin's time especially, the idea of the geometrical rate of increase of organisms has held an enormously influential place in theoretical biology. That it is entitled to such a place is beyond question—the more so the more exact and broad becomes our knowledge. So obvious—almost terrifyingly obvious at times—is the tendency of species to over-run the earth, that it is not surprising, perhaps, that field botanists and zoologists in particular should use as one of the corner stones of their biological creed the formula "every species tends to increase up to the limits of its food supply."

Do you not see that what I am driving at is a summons to a more critical attitude in relation to this element in our creed? Do the particular food habits of a species cut no figure in the way the species meets its multiplicative exuberance?

Why do men and all other animals have brains at all, and of various grades of effectiveness, if this organ counts for nothing toward saving the creatures from their greatest dangers? How did they come by this part of their organization? Is it just a free gift of Providence under the name of Evolution? As the problems of animal ecology are now almost universally stated and studied no functional difference is recognized between the nervous equipment of even the major subdivisions of the animal kingdom. The brains, for instance, of a mammal, a bird, a fish, an insect, and a mollusc are treated as of equal rank.

Surely no real zoologist—zoologist, I mean, who is concerned with animals as nature presents them—can dodge these questions even if he would like to.

But almost certainly most zoologists will be ready to come back at me with: "But what you are talking about implies psychology and not zoology. Hence, important though the question may be, we steer clear of it.

"Very well," I say, "then let's consult the psychologists." Naturally we look first to the comparative psychologists—those, that is, who are professionally occupied with the minds of animals.

The reception we get here has more of the cold shoulder about it than has that received from the ecologists. We are told, often with an undisguised smack of contemptuousness, that with such questions psychologists have nothing to do. While it is admitted by some psychologists that although such questions seem to have some relation to their science, yet since the phenomena involved are not susceptible of laboratory control and measurement, they do not come within the scope of scientific psychology. Animal psychology is concerned with the minds of animals, not with their food problems.

It is important to realize the completeness with which all problems of animal life which cannot be definitely inventoried as mental, and subjected to experimental control, chiefly in the laboratory, are now debarred from psychology, especially in this country. Nowhere have I seen this more fully set forth and insisted on than in the writings of Professor C. J. Warden of the Animal Laboratory, Department of Psychology in Columbia University. (See, for example, *A Short Outline of Comparative Psychology,* 1927, and "The Development of Modern Comparative Psychology", *The Quarterly Review of Biology,* December, 1928.)

The truth seems to be that animal psychology as now professionally conceived is distinctly more unsympathetic with, and aloof from, the problems of animal activity as these are involved in animal life under the conditions of nature, than is ecology.

I must now touch upon my suggestion about a revised kind of research. "Revised kind", I say, rather than a brand-new kind because I do not by any means want to imply that the untilled field referred to is as wholly untilled as my treatment so far would indicate. The truth is, a great deal of excellent research is being done of the general sort I am longing to see more of. But here is a queer thing about it: What is being done is done largely by amateurs—by persons, that is, who have little or no standing among the scientifically elect. I will illustrate by referring to an instance right here in California.

The Michener family of Pasadena, mother, father and two growing sons, are making studies on their dooryard birds that are thrilling to all the family and could hardly fail to appeal strongly to all zoologists whose interests are in animal life in its full scope. In fact, I venture the opinion that results they are getting on the House Finch, especially, in bringing to light individual differences of their actions, are of first rank importance from their bearing on the general problem of personality. Not many aspects of human life are receiving more attention today, and are more urgently in need of deeper, more comprehensive study than that of the nature of personality. One of the gravest defects in understanding here is due, I am sure, to lack of anything approaching an adequate study of the problem from the comparative standpoint. I know of few if any instances of a more decisive move in the direction of such a study among animals in nature than in this Michener study of the House Finch.

It would be easy to mention many studies made by naturalists in various parts of the world that have much of the character of the research I am advocating. By way of illustration from the side of publication, I mention *British Birds,* edited by H. F. Witherby. The twenty and more volumes of this academically unsponsored journal are a real storehouse of observational material of the kind needed. And our own CONDOR and many of the University of California Publications from the Museum of Vertebrate Zoology are very much to the point.

What, then, do I mean by a revised kind of research? In what respects do studies which bring certain results of the right kind need revising? First and foremost in this respect: They need to be made on a much better understanding than now prevails of what the rather hackneyed words, the "animal mind", really mean. I have no longer the least doubt that the basic trouble here is the old, old bugbear, the mind-body problem. It is a hold-over of the doctrine that the Body is one thing and the Mind is an utterly different thing; that the body is something profane while the mind is something sacred.

Although great things have been accomplished during the last few decades toward ridding civilized mankind of this disease, there is much to do yet before a

complete cure is effected and the predisposing conditions are removed. And truly I do not believe any class of scientists is better situated and better minded just now for helping forward this aspect of man's salvation than are field naturalists, especially field zoologists. Surely no other class is so favorably situated for dealing with the comparative aspect of the problem. And it must be insisted that no problem of living nature, of human nature by no means excepted, has ever been solved even approximately without much application of the comparative method.

I earnestly suggest that all of you, as you go into the field this summer, whether birding, mammaling, reptiling, or what-not, set apart from your schedule of work, probably already made, a little time to ask seriously of some one or a few species, the question I have made the center of this paper: How does this species—how do the individuals of it—use their heads in solving the problems which from day to day they must somehow solve, or, as to some of them, suffer the death penalty for not solving it? Of course my suggestion does not imply that you should neglect legs, feet, wings, body and the rest. The whole thing must come into the reckoning, surely. But the role of the head, as I have defined it, is the major point of the discussion.

Put aside, for a while, all your long-cherished though perhaps rusty or otherwise decrepit notions about "instinct", "intelligence", "thought", "mind"; about "natural selection", "survival value", "margin of safety", "trial and error", "tropisms", "standard of efficiency" and so on, and just ask: "this particular individual or close-at-hand group of individuals, how does it—or they—use its or their eyes, ears, mouths, brains in this particular food or water problem, or this particular danger or reproductive situation?

Probably you will not get far toward answering your questions in the time at your disposal. But I greatly hope that some of you may go far enough to convince yourselves that, given time enough, situations well enough selected, and skill and patience enough, it would be possible to go far toward some, at least, of your desired answers.

This brings me to my last two points: (1) The importance of school and college preliminary training in a field technique fitted to the enlarged, more philosophic-psychologic conception of zoological research.

Beyond a doubt, to my thinking, it is bound to be recognized sooner or later that training in zoology can no more be adequate without field work of the general kind I am talking about, than training in geology can be adequate without field work. Really, zoology with problems of the kind I am calling attention to left out, is no more full-rounded zoology than geology with stratigraphy together with erosion and sedimentation left out would be full-rounded geology. And think of the absurdity of supposing these earth phenomena may be studied as far as they need studying in the laboratory! Unquestionably, laboratory researches contribute much and in many ways to the solution of problems in both sciences. But to assume that field research in either may be dispensed with entirely or even largely would be unmitigated folly for either science, as much for the one as for the other.

Undoubtedly, what I am contending for implies several quite radical departures from prevailing curricular practices of both school and college education in zoology.

This leads to the other of my two final points: (2) Carefully selected field places for student instruction would be needed. Among the requisites of such localities would be the character of material available and accessibility. There is

no scarcity of good localities in such a Providence-favored country as California. Yet even here there is plenty of chance for the exercise of wisdom in choosing.

The only particularizing on this matter I have time for concerns the National and State parks. The admirable nation-wide move now under way to utilize the parks for educational purposes is directly in line with what I am contending for. But the move would need an enlargement of aims. I think I am right in understanding that at present the aim does not go much beyond that of enhancing the popularity of vacation outings; of making out-of-door recreation richer intellectually and esthetically; and of affording certain facilities to special students whose problems may at times call for work in such localities. This is all excellent. Not for a moment would I advocate a procedure that would minimize the value, or impair the effectiveness, of these aims. What I have in view is in addition to this.

The major impulse to what I conceive essential for getting the greatest possible good out of natural knowledge would have to be with the institutions of research and formal education. And central in the general purpose would be the laying of a foundation in the nature of things for the understanding of the nature of man himself. All effort in scientific research like all other human effort is motivated, finally, by human desire for human good. Sooner or later this vital truth must be seen with greater clearness and specificness than as yet it has been seen. Wherein lies the truth and wherin the falsity of the familiar sayings, "science for its own sake", "art for its own sake", and so on, must be discovered by deep and broad analysis of human nature and all the rest of nature.

In closing I cannot refrain from alluding to an incident on the campus of the University of California here in Berkeley that may contribute to what I am speaking in behalf of.

The enormous structure now taking shape just below California Hall is to be known as the Life Science Building. Thirteen (at last accounts) of the sciences of living nature are to be housed under this ample roof. That is a fine plan—fine in various ways. For one thing, it will bring together in one departmental library a great mass of literature which naturally has innumerable criss-crossings that must be artificially torn asunder if, as at present, the several departments are housed in different buildings on different parts of the campus.

Another fine thing about it is that it tends to fix attention on the question of what a life-science really is, and how many such sciences there are. And here comes my chance for turning the incident to my purpose. Since I have had no part in planning the building I am quite free to express any conjectures I may have about any aspect of the business.

Why, I wonder, "life science" rather than "biology" in choosing a name? The list of sciences to be housed here I notice contains psychology. This fact suggested to me instantly on learning it: Here is the explanation of "life science" instead of "biological science". For, I thought, how could psychology consent to be shut up in the same building with biology as latterly biology has defined itself?

According to that persistent fraud of a doctrine of the fundamental enmity between Body and Mind, biology has been the science of the Body, that is, a profane science; while psychology has been the science of the Mind, that is, a sacred science. Undoubtedly psychology is now moving rapidly away from its tradition in this matter. But surely no psychologist needs to be reminded of the tenacity with which people and things cling to their traditions.

Furthermore, we all know (or believe we do) much more about our minds than about our bodies, and prize (or suppose we do) our minds much more highly

than our bodies. Hence, psychology has seemed a much more important, a much nobler science than biology.

But, now, neither psychology nor biology could well feel any stigma from being identified, in a general way at least, with efforts at getting knowledge of natural objects that are alive. Even though some subdivisions of biology appear to be more interested in "living matter" than in living beings, yet the recognition by such biology of the fact that a certain kind of matter is alive gives that matter a peculiar interest.

Hence it is that psychology, of even the old-fashioned psychical kind, should be able to live and work quite comfortably in the same building with, let us say, biochemistry and biophysics, it being agreed that the building is called a life science building.

So I end with a hint as to what biochemistry and psychozoology may accomplish by strengthening each other's hands to the full extent of their ability. For this I return as usual to my woodpeckers. These birds eat acorns, you remember. Examination by experts of the Biological Survey, of the stomach contents of some specimens taken last fall by Mr. W. H. Burt and myself, proved that the birds were then living almost exclusively on this food. There was hardly a trace of any other food material in these stomachs.

Now how under the sun could I learn as much as I would like to about the composition and food value of acorns, and the digestive processes which the woodpeckers put them through in utilizing them as food, otherwise than by depending almost entirely on what biochemists can tell me? Their help is simply indispensable to my project as a whole.

Can I do anything for them in return? Well, to collect materials for their analyses is something, not much to be sure, because they could with a little trouble do that for themselves. But if they will go into the field with me and stay long enough and travel widely enough I will show them that the birds manage somehow to fabricate a lot of things, organs and tissues, which laboratory analyses do not throw much light on. But what is much more, the birds manage somehow to get a lot of energy from the nuts by means of which they accomplish a lot of things which chemical analysis throws hardly a ray of light on so far as details are concerned. In other words, I will undertake to convince the biochemist that by studying the activities of the California Woodpecker he can learn something about the chemical and physical properties of some of the substances of acorns that he cannot possibly learn from his laboratory researches.

Intensive cooperation between psychozoology and biochemistry can teach us a great deal about the causal efficiency of much of the natural order, both as to the animate and the inanimate parts of that order, that we can not learn in any other way.

Why not, then, make the beginning of this working together in the same building by all these sciences of living nature the beginning also of a more vital cooperation of these sciences than they have ever yet entered into for answering such queries as this of mine: In what ways and how successfully do men and all other animals use their heads to promote their own good?

University of California, Berkeley, May 24, 1929.

A NEW CORMORANT FROM THE MIOCENE OF CALIFORNIA

WITH TWO ILLUSTRATIONS

By LOYE MILLER

In June of the year 1928, Mr. Dan J. Poyer of Calabasas, California, placed in my hands two slabs of a fine grained shale, from his rock quarry, which contained the major portion of an avian skeleton very beautifully preserved. One slab was the property of Dr. Frederick Kellogg of Los Angeles, the other was presented by Mr. Poyer to the University of California at Los Angeles. The two specimens (figs. 58 and 59) represent the obverse and reverse of the same individual split along a bedding plane of the shale, and they are mutually supplementary.

Mr. Poyer's quarry is located near Calabasas, nw. ¼ of sec. 18, T. 1 N., R. 17 W., in what is designated as the Modelo formation by Kew, and considered to be of late Miocene or early Pliocene age. Accompanying the bird remains in the quarry are numerous fish remains of various sizes and some very beautiful specimens of marine algae. The matrix is of light grayish tone and the fossil is rich brownish or even black where the tarsal envelope, the foot webs, and the feathers have left their imprints recorded.

The bird is a totipalmate closely approximating the modern cormorants. In size it falls between the smaller *Phalacrocorax pelagicus* and the larger *Phalacrocorax penicillatus,* approaching closely an adult female specimen of the latter species. In a study of the osteological characters, however, there appear a number of points of divergence from the available specimens of recent species. For reception of the Miocene bird a new specific category is proposed as follows.

Phalacrocorax femoralis, new species

Size slightly less than smallest *Phalacrocorax penicillatus,* from which it differs also in having longer and straighter femur, much shorter tibia, shorter and heavier tarsus, broader pelvis and smaller sternum.

Sternum.—One of the outstanding features of the species is the small size of the sternum in comparison with the pelvis. The latter element is fully as broad throughout the entire length so far as preserved, as in a medium sized *Phalacrocorax penicillatus,* yet the sternum is less than three-fourths (74%) as broad or as long. This difference is not such as to indicate a loss of the power of flight, and the coracoid, the humerus, and the wing primaries are well developed. Taken with the long femur and stocky tarsus, however, the suggestion is that flight was resorted to even less than with our modern cormorants. Possibly fishing excursions extended habitually less far from shore.

Pelvis.—The pelvis itself appears remarkably broad as compared with the modern cormorants. This feature obtains in the post-acetabular region as well as in the peculiar butterfly expansion of the iliac crests. Fortunately this latter characteristic region is perfectly preserved underneath the superimposed sternum. By sacrificing the right half of the sternum the ilium could be worked out with a dental tool under the microscope, leaving the left half of the sternum and its extreme right border intact. Both sternal and pelvic regions are decidedly cormorant-like. The entire ischial and pubic portions of the pelvis have been lost and the posterior extremity of the ilium can only be approximated, yet this approximation is felt to be fairly accurate.

With this small factor of uncertainty, the total length of the pelvis compared with that of the Brandt Cormorant is 83 per cent, while the width of the anterior

expansion of the iliac region is 100 per cent and the width across the ischiatic foramen is 142 per cent. This latter ratio is not thought to be the result of post mortem crushing since there appears no distortion in the transverse section in this or in other parts of the skeleton. The pelvis of *Anhinga* displays this character of broadness, but otherwise the resemblance is more remote. The posterior half of the fossil pelvis is less gracefully formed, being more nearly rectilinear than that of either the cormorant or *Anhinga*.

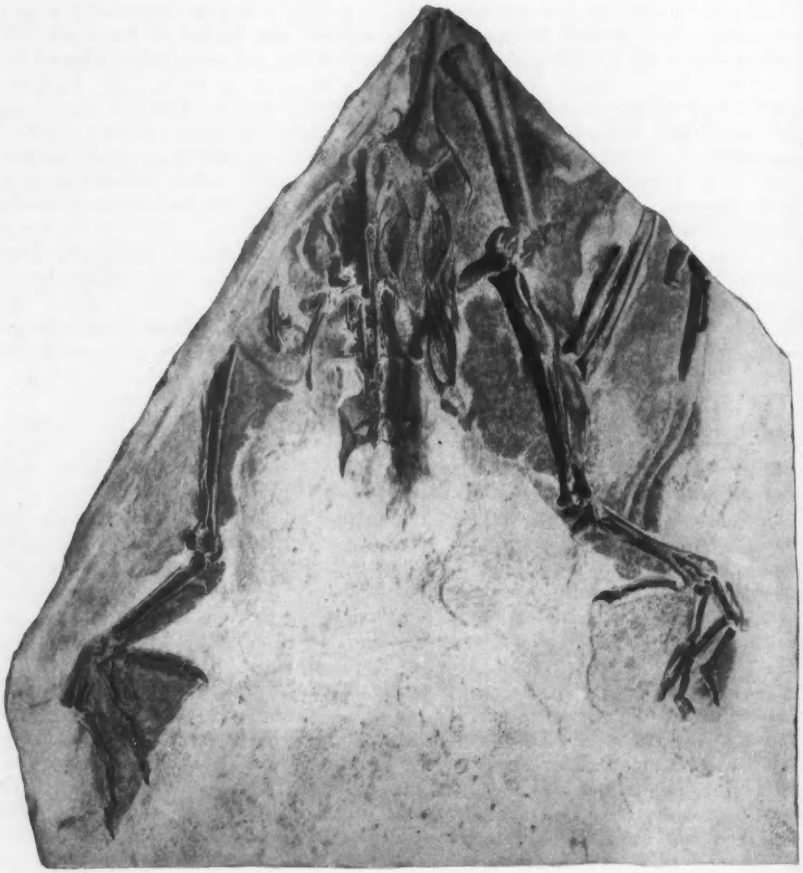


Fig. 58. SLAB CONTAINING PART OF SKELETON OF *Phalacrocorax femoralis*, NEW SPECIES.

Foot.—The foot is decidedly cormorant-like so far as preserved. The long hind toe included within the web, the deep tarsal sheath, and the very prominent hypotarsus extending down the tarsal shaft in broadly curved profile are char-

acteristic. Unfortunately the outer toe is not preserved in either foot, but toes I, II, and III stand in the following length ratios: 100, 118, 206.

In the Brandt Cormorant, the corresponging parts stand in the ratios of 100, 145, 217; in the Farallon Cormorant, 100, 147, 189. There seems to be a marked brevity of the second toe in the Miocene bird.

In the process of quarrying the stone, both tarsus and tibia were split longi-

Fig. 59. SLAB CONTAINING REVERSE OF SKELETON, THE OBVERSE OF WHICH IS SHOWN IN FIGURE 58.

tudinally; thus all except the grosser characters were destroyed. The left femur is preserved for over half its length and the right is again seen in longitudinal section. Both show a lack of curvature that is sharply in contrast with the Recent

cormorants. The intermuscular lines on the posterior surface of the proximal portion are practically identical in the two species.

An excellent imprint of the patella is available in the Kellogg slab. The part differs markedly from the true cormorants in being much broader but not so high, the effect being much like that of the broad cnemial crest of a shearwater and quite in contrast with the high pyramidal patella of a cormorant.

Humerus.—The right humerus is sufficiently well preserved to show the family characters of the proximal region including the low deltoid crest, the sharp angle at the insertion of the pectoralis minor, the depression at the proximal extremity of the bicipital furrow, and the general rectilinear effect of the deltoid profile of the entire bone. The distal end is almost entirely obscured by being tucked under the knee. The total length, however, is determinable, and it almost coincides in this dimension with *Phalacrocorax penicillatus.* In view of the stronger pelvis and weaker sternum, this well developed humerus is of interest.

The remaining segments of the wing are not preserved to sufficient extent to be distinctive in any way.

Coracoid.—There is a close approximation in size of the coracoid with that of *Phalacrocorax penicillatus,* but positive differences appear in the contours both of the base and of the head of the bone. The central profile as the bone is viewed from the ventral aspect, is less concave basally and more concave toward the head, the latter point being due to more pronounced clavicular facet, the former to a shorter base of contact with the sternum. Again, this sternal facet is placed at a more acute angle with the shaft of the coracoid, and lies more nearly under the main axis of the bone.

The total effect of these profiles is a coracoid that is more symmetrical with respect to the main axis of the bone.

TABLE OF MEASUREMENTS

	Calabasas Cormorant	Farallon Cormorant
Tarsus—Length	56.5 mm.	64.5 mm.
Depth hypotarsus	16.5	17.
Depth shaft	6.7	5.6
Tibia—Length	93.	162.
Across condyles	14.	12.5
Shaft width	6.3	7.
Femur—Length	56.5	57.2
Head	15.5	15.
Shaft	7.9	7.6
Humerus—Length	136.2	145.2
Shaft	8.2	7.7
Head	22.	22.
Sternum—Length median line	59.3	72.6
Length lateral line	56.5	68.
Width posterior end	56.	52.3
Coracoid—Width base	27.5	26.5
Sternal facet		20.
Pelvis—Width across acetabula	31.	33.6
Width across ischia	22.	44.5
Hind toe—Total length	32.7	38.
First joint	21.3	
Inner toe—Total length	48.8	51.
Middle toe—Total length	69.	66.

Discussion.—The entire assemblage of fossils, both plant and animal, would

indicate accumulation in quiet water, free from any appreciable disturbance by wave or tidal current. The many marine algae seem to be "floated out" as carefully as the collector of "sea mosses" floats them out upon his mounts. Tidal currents or strong wind ripples tend to roll such drift into knots or windrows. The nearly normal association of parts in all fish and bird skeletons uncovered indicates a similar lack of any marked disturbance.

Matrix.—The matrix is extremely fine grained and homogeneous, splitting into thin lamellae. Under the microscope, there appear the tests of diatoms and fine spicules of sponges. Samples were subjected to chemical and optical analysis by a capable geology student, Miss Mary Kathryn McGee, who made a special search for foraminifera. Her report is as follows:

The rock at first glance, appears to be an ordinary siliceous shale. Fresh surfaces are cream colored and the rock splits fairly evenly along well defined bedding planes. Weathered surfaces are pitted and give the impression of a secondary deposit covering the fresh rock. As a whole, it is only slightly stained by iron oxide.

On closer examination, however, the freshly cleft rock is seen to possess a number of odd characters. The surface is marked by numerous minute knob-like areas, with corresponding depressions between, giving the impression of an imperfectly formed oolite. Moreover, fresh surfaces, on minute examination, possess a pearly lustre that gives them a polished appearance.

Microscopic examination of thin sections shows the rock to be composed principally of calcite, diatom frustules, and siliceous sponge spicules. A few patches of secondary quartz are also present. In all the slides examined, only one recognizable foraminifer could be found. This was of the genus *Bolivina*. No other faunal forms are present.

There was no indication whatever of oolitic structure in the sections, but there is very evident accumulation of the calcite in fairly definite zones about many diatoms, as well as a filling of the tests by the same mineral. I believe that the peculiar knob-like appearance of the rock surfaces is to be explained by this accumulation of calcite about the diatom frustules. Moreover it is more logical to believe that this accumulation is a secondary phenomenon rather than a primary oolitic type of accretion upon the frustules at the time of their deposition. That is, the calcite, which probably was deposited originally in the form of aragonite, aided by subsequent consolidation of the sediment, has migrated during the process of recrystallization and has filled the once empty tests. The mode of precipitation of the calcite is doubtful, although it could have been due to bacterial activity.

Chemical analysis of a sample shows the rock to contain about 9.8 per cent alumina. This is probably in the form of kaolin and represents a fine silty deposit in relatively quiet water some distance from shore. The rock has evidently undergone sufficient regional metamorphism to change at least a portion of the kaolin to kaolinite and this would account for the pearly lustre.

The rock, then, is an argillaceous-diatomaceous shale with considerable lime content which represents an original deposit concurrent with the accumulation of the diatoms and sponge spicules. The material probably represents a relatively shallow inland sea deposit rather than an estuarine one, as indicated by the presence of the sponges.

Judging from the locality from which it was taken, it undoubtedly belongs to one of the more siliceous members of the Modelo series. The formation that outcrops in this vicinity, according to Kew (U.S.G.S. Bull. no. 753, 1923), is one which has been correlated with the Modelo formation of Santa Clara valley.

The accumulation at Calabasas differs from that at Lompoc in being a hard oölitic limestone with smaller admixture of diatomaceous silica instead of the light, siliceous shale, almost a pure diatom accumulation, that we find at Lompoc. The two deposits are similar in their fine grain and thin lamination, in their freedom from detrital material, and in the undisturbed condition of their contained fossils.

Both must have accumulated in land-locked, quiet basins free from the contribution of any large stream. The Calabasas matrix may have been laid down at a slightly later period geologically.

The bird here newly named differs from the surviving cormorants by characters that are not found in the genus *Phalacrocorax,* but they are held to be not of great significance, and to establish a new superspecific category at present would result in a monotypic genus, the thought of which is not attractive to the present writer, who holds that the genus should indicate relationship by being inclusive and that the species as groups should emphasize differences. Especially does this view appeal to one handling the extinct forms where the record is so discontinuous that the monotypic genus would be the rule were we to adhere closely to the standards of the extreme systematist in modern ornithology. Should additional and related material be found at some future time, the establishment of a new genus might become advisable.

University of California at Los Angeles, May 18, 1929.

ON THE SUBSPECIFIC VALIDITY OF ANSER GAMBELLI HARTLAUB

WITH THREE ILLUSTRATIONS

By NAGAMICHI KURODA, Sc. D.

In the past, *Anser albifrons albifrons* (Scopoli) (1769—type locality: North Italy) and *Anser albifrons gambelli* Hartlaub (*Anser Gambelli* Hartlaub, 1852—type locality: Texas and southern United States; synonym, *Anser frontalis* Baird, 1858—type locality: Interior of North America, juv.!) have been known as two subspecies of *Anser albifrons*. Some ornithologists (Lord Rothschild, M. Delacour, and others) consider that *gambelli* is based merely upon larger individuals of *albifrons*, Dr. Hartert writes that it is a doubtful form of *albifrons*, and some (Swarth and Bryant, and others) believe that it is a good subspecies.

Hartlaub (1852) described three specimens from Texas and southern parts of North America as follows:

Anser Gambelli, Nob.—(Notice provisoire.)—*Synon.* Anser albifrons Americ. septentr.

Nous avons examiné trois exemplaires de cette espèce d'Oie, dont deux provenaient du Texas et l'un du sud de l'Amérique du nord. Ce dernier est *presque* adulte; les deux du Texas sont des jeunes. L'énorme grosseur et la forme différente du bec nous force de séparer cette espèce de notre *albifrons*. Voici les dimensions comparatives:

	A. Gambelli		A. albifrons	
Longit.rostri a fr.	2″ 4‴	[= 59.3 mm.]	1″ 6‴	[= 38.1]
A rict.	2″ 4‴ ½	[= 60.3]	1″ 8‴	[= 42.3]
Altitud. rostri later.	1″ 2‴	[= 29.6]	10‴ ½	[= 22.2]
Circumferent.rostri ad bas.	3″ 6‴	[= 88.9]	2″11‴	[= 74.1]
Longit. tars.	2″ 8‴ ½	[= 68.8]	2″ 2‴	[= 55.0]
Dig. Med.	2″10‴	[= 72.0]	2″ 6‴	[= 63.5]

Le congrès des ornithologistes, à Berlin, en 1851, a approuvé la séparation spécifique de cette Oie américaine.

It is a very regrettable fact that the above description of Hartlaub does not include the wing measurements.

Last year (1928), through the courtesy of Dr. Stresemann, of the Zoological Museum, Berlin, I was shown in that institution three mounted birds which are said to have been Hartlaub's specimens. As no particular one had heretofore been selected as the type of *Anser gambelli*, Dr. Stresemann in my presence so designated one of the three (Zool. Mus. Berlin Coll. no. 17430).

Measuring these birds myself, I found the measurements much smaller than those given by Hartlaub and wholly identical with those of *albifrons*. My measurements, in millimeters, are as follows.

Zool. Mus. Berlin No.	Locality	Date	Sex		Total length (about)	Exposed culmen	Bill from gape	Tarsus	Middle toe and claw	Wing	Tail	Greatest height of bill at base
14942	"Nord America"	.....	"Ad."=♂?	juv.	735	50.5	53.5	67.5	67.5	395	106.5	29.5
17430 (type)	"Alvarado, Texas"	Jan., 1828	"Juv."=♀?	ad.	705	52.5	51	66.5	71	404	122	28.5
17431	"Alvarado, Texas"	.....	"Juv."=♀?	ad.	710	52	53	75.5	77.5	408	119.5	29

The newly selected type specimen is a very old bird, having most of the underparts covered with black patches of large size, with a rather smaller bill than in

the young male (no. 14942) that Hartlaub seemed to consider as an adult. The latter bird seems to be a young of the year, as it has no black spots on the underparts, and the chest is slightly marked with shaft spots of grayish brown (juvenal feathers). The white frontal patch is small in area and the feathers at the base of the upper mandible have a faint, white linear patch only. There is a distinct blackish brown patch at the base of the upper mandible, close to the whitish line. No. 17431 seems also an adult female (but not an old bird) with fewer black spots (only five in number) on underparts. My measurements (given above) show that they are all three typical *albifrons*.

It is very doubtful that there would really be such great differences (10 mm.) between Hartlaub's measurements and mine of exposed culmen and length of bill from gape, if taken from the same bird. The discrepancies are too great to be explained as due to differences in methods of measurement followed by different people. Questions now suggest themselves that may be summarized as follows.

1. Was Hartlaub's description really based upon the three birds examined by me, and supposed to have been his specimens?
2. Were not the original type specimens of *gambelli*, described by Hartlaub in 1852, replaced by the above specimens since then?
3. Is it not likely that the original types are at some other place than the Berlin Museum?

I think that the doubts I express are reasonable; for the two forms, *albifrons* and *gambelli*, do migrate in winter to Texas and other southern parts of the United States, as stated by Swarth and Bryant (1917).

Last year (1928) I examined specimens of *Anser albifrons* in several museums in Europe and America. At the Berlin Museum there are only the above mentioned three *albifrons*. In the several museums in North America most of the birds labeled as "*gambeli*" are typical *albifrons*. I measured some examples from Korea at the United States National Museum, Washington, D. C., which had been reported by A. H. Clark as "*gambelli*". In these birds the exposed culmen varies from 49 to 55 mm., being of no larger size than in *albifrons*.

At the Naturhistoriska Riksmuseum, Stockholm, I examined the head of an adult collected in Kamtschatka in 1927. In this bird the exposed culmen is 51.5, and height of bill at base 28 mm., as in typical *albifrons*. There is one skin and one head preserved in the British Museum (Natural History) that are of great size and coincide with the description of *gambelli*. Their dimensions, in millimeters, are as follows:

Brit. Mus. Reg. No.	Locality	Date	Sex	Exposed culmen	Bill from gape	Tarsus	Middle toe and claw	Wing	Tail	Greatest height of bill at base
1848.3.13.112	"Repulse Bay"	.....	Ad.	61.5	62.5	78	80	425	129	30.1
1892.2.1.365	"Arctic Coast, E. of Ft. Anderson"	6.vii.1865	♀ ad.	59	60.5	....	....	.....	.....	31.5

These two are evidently larger birds than the three preserved in the Berlin Museum.

On a visit to the Museum of Vertebrate Zoology, University of California, Berkeley, in 1928, I examined a large series of geese labelled as "*Anser albifrons*

gambeli". Later, through the kindness of Dr. Joseph Grinnell of the said museum, one of the finest adult males was sent to Tokyo, together with measurements of the entire series. The specimen received bears out the conclusion of Swarth and Bryant (1917) that two forms (larger and smaller) occur in North America. While I thus agree with the general proposition advanced by those authors, still after my careful examination of specimens, I differ from them in certain particulars, as detailed beyond.

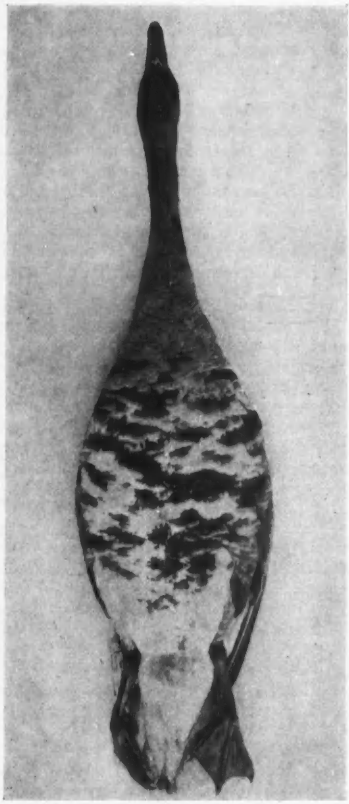


Fig. 60. SKIN OF AN ADULT MALE TULE GOOSE (*Anser albifrons gambelli*) COLLECTED ON BUTTE CREEK, SUTTER COUNTY, CALIFORNIA, NOVEMBER 19, 1916; MUS. VERT. ZOOL. NO. 27134.

In general appearance the so-called "Tule Goose" is similar to *albifrons*, but it is distinguishable from it by enormously larger size. The tarsus of the Tule Goose is much thicker and longer (width in central part 16 mm. in dried state instead of 11 to 12.1 mm., as in *albifrons*; length of tarsus 84.5 mm. instead of under 79 mm., as in the latter form). The bill of the Tule Goose is thicker; nails on upper and under mandibles wider and longer (length of nail on upper mandible 16 mm., that of lower mandible 15.5 mm., instead of 13.1 to 14.1 on upper mandible, 10 to 12 on lower mandible, as in *albifrons*). The wing is on an average longer. The bill is rather less slender than that of *albifrons*, and length of exposed culmen including nail on maxilla of adult male is 3½ times that of nail, as in females of *albifrons*, instead of 4 times, as in adult males of the latter form.

Coloration is essentially as in *albifrons*, but the entire crown of head and nape is very dark umber brown, nearly black, blacker than most examples of *albifrons*. Feathers at base of both mandibles near the white patch and throat are much blacker. The above-mentioned characters are taken from an adult male from Butte Creek, Sutter County, California, November 19, 1916; Mus. Vert. Zool. no. 27134; shot by Geo. Neale.

This specimen was measured by myself as follows:

Wing, 468 mm.; tail, 146; tarsus, 84.5; exposed culmen, 58; anterior end of nostril to tip of bill, 32.5; bill from gape, 62; height of mandible at base, 32.4; middle toe, 78; middle toe and claw, 92. Number of "teeth" on one side of upper mandible, 27. Number of tail-feathers, 16.[1] The total length is given by the collector as "854 mm.", and the spread of wings is "1667 mm."

[1] Editorial note: This bird, which was skinned by myself, appeared to have lost some tail feathers. See Swarth and Bryant, 1917, p. 212.—H. S. Swarth.

As the result of my examination of this adult male of the "Tule Goose", I agree with Swarth and Bryant in their assertion of the occurrence of two forms of *Anser albifrons* in North America.

The opinion of Dr. Hartert, of Tring Museum (Vög. Pal. Fauna. II, pp. 1281-1282), on the validity of *Anser gambelli,* is, I think, very important, so I insert herewith the English translation, as follows:

In the Cat. B. Brit. Mus., XXVII, p. 95, and in the A. O. U. Check-List . . . . , the white-fronted geese of North America and eastern Asia (Japan) were separated on the basis of greater size, especially of the bill. As Alphéraky in "Geese of Europe and Asia," p. 46, previously stated, and examination of the series in the British and Tring museums and some smaller private collections confirmed, by no means all American and very few eastern Asiatic specimens are greater than European ones; on the other hand, I have examined very small examples from Japan and Korea.[3] *Gambelli* could, therefore, be placed in the synonomy of *albifrons*, since it is not [only] in North America that surprisingly large specimens with bills to 60, wings to 448 and 475 mm. are produced; whilst I could point out no example from eastern Asia that would not thus be equaled from western Europe. The probability is, nevertheless, that in the far north of eastern America a large billed form exists, as a series of breeding birds from Greenland and Iceland would show.

After this was written, there appeared an article by Swarth and Bryant in Univers. California Publ. in Zool., XVII, No. 11, pp. 209-222, with illustrations, in which the white-fronted geese of North America are critically treated. The professors come to the conclusion that two forms exist there, *A. albifrons albifrons* and *A. albifrons gambelli*; that they both meet in their winter range; but that the latter have a very much more easterly breeding range. They believe that the two are not only separable by the bill dimensions, but also that *A. a. albifrons* is lighter, particularly the head and neck being grayer, the bare eyelid grayish brown, the rectrices 16, wing 384 to 422, culmen 44-52 mm.; on the other hand, that *A. a. gambelli* is larger, the coloring on the whole darker, the neck brown, the head blackish, the naked upper eyelid yellow or orange, rectrices, male, 18; female, 16; wing 420-475, bill 53 to 62 mm. These points I am unable to verify. The coloration is variable, as brownish and grayish examples are found in Europe; the characteristic is thus not unqualifiedly useful. The upper eyelid of one specimen obtained in Ireland I found dark yellow! Naumann called it reddish yellow or also only reddish gray. Also it is not correct that all the smaller white-fronted geese have only 16 rectrices; 4 or 5 times I found European males with 18 rectrices, also a similarly bedecked female, which also was certainly slightly larger than a female should be; Naumann also found 18 once. Two subspecies can be recognized first when, the breeding place being established, both forms have separate nesting grounds.

In agreement with Dr. Hartert I can criticize some details of the conclusions of Swarth and Bryant, but as regards Hartert's doubt of the occurrence of two forms in North America, that matter seems to be settled beyond question. A specimen in the British Museum is of the utmost importance in this connection, an example of the larger form, collected July 6, 1865, on the "Arctic Coast, E. of Ft. Anderson". I presume that that goose was obtained on its breeding ground, and it supplies proof of the hypothesis of Swarth and Bryant that *gambelli* might be found to breed in Arctic America east of Alaska.

The statement of Swarth and Bryant that the naked skin at edge of eye-lid of the "Tule Goose" is yellow or orange, and that of *albifrons* is grayish brown, is not invariably true, as Dr. Hartert points out. I once had in captivity a Japanese example of *albifrons* with yellow eyelids, but I think this condition is rare in

[3] Clark (Proc. U. S. Nat. Mus., 38, 1910, p. 151), however, called Korean specimens *A. a. gambelli*, without, nevertheless, specifying the reasons.

albifrons. Regarding general coloration, Dr. Hartert may be correct, but the crown of the head in the one specimen of "Tule Goose" at hand, lately sent by Dr. Grinnell, is of the blackish color described by Swarth and Bryant, much blacker than in any skins of *albifrons* secured in Japan and Korea. After all, the difference between the two forms is remarkable, in size of body, in length of wing, and in

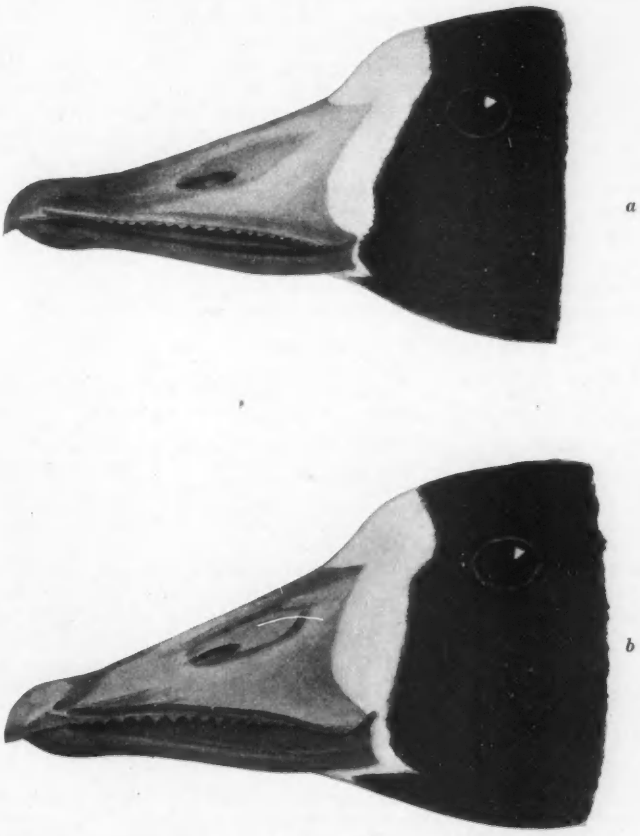


Fig. 61. *a*. HEAD OF ADULT MALE OF *Anser albifrons albifrons* (ONE OF THE LARGEST YET OBTAINED IN JAPAN); N. KURODA COLL. NO. 39. *b*, HEAD OF *Anser albifrons gambelli*; MUS. VERT. ZOOL. NO. 27134. BOTH FIGURES NATURAL SIZE.
Drawn by Mr. S. Kobayashi.

thickness of tarsus. As regards number of tail-feathers, I cannot accept the statement of Swarth and Bryant, for *albifrons*, male and female, often has 18 tail feathers, and at the same time the male "Tule Goose", no. 27134, Museum of Vertebrate Zoology, has only 16 feathers! [But see footnote, p. 175.]

The last question remaining to discuss is the scientific name to be given to the

big form of *albifrons* in North America, the so-called "Tule Goose" or "Timber Goose". Messrs. Delacour and Hachisuka, who visited the Museum of Vertebrate Zoology, Berkeley, two years ago, examined the series of these large geese in that collection. Their conclusion, as they told me, was that if the types of *gambelli* in the Berlin Museum are not different from typical *albifrons,* then these birds represent an undescribed form. Though I have investigated the three geese in the Berlin Museum and am convinced that they are all really *albifrons,* I am still compelled to believe Hartlaub's original description. Until the questions that are put at the beginning of this paper are solved clearly, I shall call the "Tule Goose" *Anser albifrons gambelli* Hartlaub (not *gambeli* as by many American authors). I beg of any persons who are interested and who may have information bearing upon the question as to whether or not the assumed type specimens of *gambelli* in the Berlin Museum are really such, that they will communicate with me or else publish their findings themselves. If those birds really are the original specimens, and the measurements in the original description of Hartlaub are determined to be mistaken, then I believe that the name *Anser frontalis* Baird (1858) given to two young examples (from Selkirk settlement and Fort Thorn) may be applied to the larger American subspecies of *Anser albifrons,* the "Tule Goose".

The comparative measurements of both forms are as follows.

Adults of *Anser albifrons gambelli*[1] from California

Sex	Wing	Tail	Tarsus	Exposed culmen	Height of bill at base
8 ♂ ♂	430-475	124-144	80-84	54.3-62	28.8-33.7
3 ♀ ♀	422-443	131-141	78-81	51.2-58	28.8-32.5

Adults of *Anser albifrons albifrons* from Japan and Korea.

Sex	Wing	Tail	Tarsus	Exposed culmen	Bill from gape	Height of bill at base	Number of tail feathers
9 ♂ ♂	380-440	118.5-135	65-79	46.5-57.5	53-59	26.5-30	16-18
17 ♀ ♀	368-419	118.5-133	64-76	43-53	49.9-54.5	25-29.5	16-18

As shown by the above measurements, specimens of *albifrons* from Japan and Korea are on the average larger than those from Europe, and it seems probable that some from eastern Asia show increase in size, as mentioned by Alphéraky. But in Japan, there are very few larger ones, old males (or very exceptionally old females), and there are none to be compared with the "Tule Goose" of North America. The largest examples of ♀ and ♂ from Japan, show the following measurements: wing 419, 440; exposed culmen 53, 57.5; tarsus 76, 79. The tarsi especially, in both sexes, are shorter than in *gambelli.*

A key to the two forms may be constructed as follows:

Size larger (wing 422-475); tarsus longer (78-84.5) and thicker (16 mm. in skin); bill larger (culmen 51.2-62); coloration darker.....*gambelli.*
Size smaller (wing 368-440); tarsus shorter (64-79) and thinner (11-12.1mm. in skin); bill smaller (culmen 43-57.5); coloration paler.....*albifrons.*

Distribution of *A. a. gambelli.*—The specimens upon which the present paper is based are mostly from Butte Creek, Sutter County, California (obtained in November and January), one from Repulse Bay (Lat. 66°20′ N., Long. 86°30′ W.) in Keewatin, North Canada, and another from the Arctic Coast, E. of Ft. Anderson (obtained in July), also in North Canada. It seems to breed in the high

[1] Measured by Dr. Grinnell, excluding no. 27134, from a series of *gambelli* preserved in the Museum of Vertebrate Zoology, for the present paper.

a *b*

Fig. 62. FEET OF TWO FORMS OF *Anser albifrons*, BOTH NATURAL SIZE: *a*, *A. a. albifrons*, MALE (ONE OF THE LARGEST YET OBTAINED IN JAPAN); N. KURODA COLL. NO. 39. *b*, *A. a. gambelli*; MUS. VERT. ZOOL. NO. 27134.
Drawn by Mr. S. Kobayashi.

north, in Arctic North America and to migrate to the United States in winter, south to California, Texas, etc.

In finishing the present paper I have to express the deepest gratitude to Dr. Joseph Grinnell and Dr. E. Stresemann, and many thanks also to other ornithologists, as Mr. Bannerman, Mr. Allan Brooks, Mr. A. H. Clark, M. Delacour, Count Gyldenstolpe, Mr. M. Hachisuka, Mr. James Moffitt, Dr. C. W. Richmond, Mr. J. H. Riley, Lord Rothschild, Dr. Uchida, and others, who have given me every assistance. I wish also to thank Prince Takatsukasa, who has called at my house and has given me several suggestions regarding the specimen from Berkeley.

BIBLIOGRAPHY

Alphéraky, S.
1905. The Geese of Europe and Asia, being the Description of most of the Old World Species. London, Rowland Ward Ltd., x + 198 pp., 1 frontispiece, 24 col. pls.

Baird, S. F.
1858. Birds. Report of Explorations and Surveys, to Ascertain the Most Practicable and Economical Route for a Railroad from the Mississippi River to the Pacific Ocean, IX, Part II, pp. 762-763.

Hartert, E.
1920. Die Vögel der paläarktischen Fauna, Band II, pp. 833-1764.

Hartlaub, G.
1852. Descriptions de quelques nouvelles espèces d'Oiseaux. Paris, Revue et Magasin de Zoologie, 2e série, IV, pp. 1-7.

Kuroda, N.
1928. Notes on the Validity of *Anser gambelli* Hartlaub, in Review of some Japanese and Formosan Birds. "Tori," VI, No. 26, pp. 1-2. (In Japanese.)

Moffitt, J.
1926. Notes on White-fronted and Tule Geese in Central California. Condor, XXVIII, September, 1926, pp. 241-243.

Salvadori, T.
1895. Catalogue of the Birds in the British Museum, XXVII, pp. 88-89, 95-97.

Swarth, H. S., and Bryant, H. C.
1917. A Study of the Races of the White-fronted Goose (*Anser albifrons*) occurring in California. Univ. Calif. Publ. Zool., vol. 17, no. 11, pp. 209-222, pl. 13, 2 figs, in text.

Fukuyoshi Cho, Akasaka, Tokyo, Japan, March 14, 1929.

FROM FIELD AND STUDY

Immature Song Sparrow in Full Song.—On July 9, 1926, I was surprised to see and hear an *immature* Rusty Song Sparrow (*Melospiza melodia morphna*) in full song. The performance was also witnessed by Prof. M. E. Peck of Willamette University, an experienced observer. We were on the porch of a residence a few miles south of Portland, Oregon, when we noticed a young Song Sparrow attempting to sing. It was so close that there could be no question but that it was immature. At first the song was subdued in volume, as though the bird were merely trying out its voice. It flitted from branch to branch, attempting to sing a couple of times, till within about eight feet of us, then burst forth into full, unrestrained song, not by us to be distinguished from the song of an adult. This was repeated several times.—WM. E. SHERWOOD, *San Fernando, California, April 22, 1929.*

The White-tailed Kite in Orange County, California.—Recent records of the White-tailed Kite (*Elanus leucurus*) in southern California are so few that it seems worth while to publish the following observations. On February 22, 1924, a White-tailed Kite was seen in the willows near the mouth of San Juan Creek, about three miles south of San Juan Capistrano. On December 2, 1924, and again on the following day, one was seen hunting over a weed covered field, three-quarters of a mile northeast of Cypress; and on January 7 and 23, 1925, what was presumably the same bird was seen at the same place, sometimes in one of several small eucalyptus trees at the edge of the field and sometimes flying back and forth or hovering on vibrating wings. On January 6, 1928, one was seen flying steadily in a northwesterly direction, about two miles southeast of Buena Park; and on December 24, 1928, one was seen hunting over weed covered fields about two miles southwest of Buena Park.—JOHN MCB. ROBERTSON, *Buena Park, California, April 11, 1929.*

Poor-will Noted in San Francisco County, California.—On April 20, 1929, a Dusky Poor-will (*Phalaenoptilus nuttallii californicus*) was shown to a group of students by Dr. H. C. Bryant in southern San Francisco County. The bird was located under a bush, a mile from Daly City, in the canyon leading from the old pumping station to Lake Merced. It was so protected by its coloration that it was not easily distinguished. A search disclosed no nest. Finally disturbing it, a glimpse of white on the tail feathers was seen as it flew low over the bushes, alighting several times, each time on a bare spot on the ground. When flushed again, it came back to within a few feet of its starting point, completing a circle about 100 feet in diameter. There it settled down on an exposed sandy spot in plain sight of all, paying no heed to the group which approached within ten feet of it. The only signs of agitation were a blinking of the eyelids and a vibration of the throat, the latter making the white throat feathers ripple like a curtain in a breeze. There it was left, in view of all passers-by, but so camouflaged that only the quick eye of a bird student would find it.

After careful search I have found no record of a Poor-will for either of the counties of San Mateo or San Francisco. Could this have been a first instance of occurrence?—SELMA WERNER, *San Francisco, May 2, 1929.*

A Record of the Red-breasted Goose in California.—In 1893, while looking over some of the late additions to the collection of birds in the California Academy of Sciences, I saw a specimen of goose that was new to me as well as to the curator, Mr. W. E. Bryant. The label, in the well known writing of Lyman Belding, stated that it had been purchased in the local market. Mr. Bryant told me it had recently been prepared by himself and that he, so far, had been unable to identify it. There was no trouble in locating the species as *Branta ruficollis*, the habitat of which is given in the Catalogue of Birds in the British Museum as "Western Siberia, Northern Turkestan and Caspian Sea. Accidental in Europe and Egypt". Four specimens, only, were listed as in the British Museum, indicating that it was not then a common species in collections.

Mr. L. M. Loomis, who succeeded W. E. Bryant at about this time, refused to

consider the matter of sufficient value to place it on record, saying to me that it was only a cage bird, imposed on the Museum by the previous curator and it should be destroyed. Having a desire to get to the bottom of the matter, I inquired of both Belding and Bryant, asking for full particulars. It was well known to many of Mr. Belding's friends that while in San Francisco he made daily calls at the game market, to see what strange species might be found. He said that the Red-necked Brant was tied with several of the local species, received from the hunters, supposedly from the wheat fields of the country to the north of the city. Mr. Bryant said that when he skinned it he found plenty of shot in the body, indicating that if it was a cage bird someone went to the trouble to take it out and target a shot-gun on it before putting it in with the regular shipment of native geese! Neither of the above men were in doubt but that the bird was an accidental visitor to our shores and the species fully entitled to a place among our birds. The logical theory was that it had become separated from its fellows in Siberia and joined a flock of our native species, migrating south along the North American coast with them. Unfortunately, the skin was lost in the fire that destroyed the entire Academy collection, and it will never be known, further than these few notes, just the status of that specimen.—A. W. ANTHONY, *San Diego Museum of Natural History, San Diego, California, May 6, 1929.*

A Record of Tyrannus melancholicus occidentalis for the State of Washington.—The recent record by Kermode (CONDOR, 30, July, 1928, p. 251) of the taking of "*Tyrannus melancholicus satrapa* (Cabanis and Heine)" on Vancouver Island, recalls the fact that there is in the Dickey collection at the California Institute of Technology, a male of the year of *Tyrannus melancholicus occidentalis* Hartert and Goodson, collected by Carl Lien in "Jefferson County", Washington, on November 18, 1916. It is now no. 22269 of the Dickey collection. This specimen was purchased by Mr. Dickey from Paul Trapier as part of a general collection of Washington birds mostly taken by Mr. Lien. It was labelled by the original collector as "Ash-throated Flycatcher".

The specimen here recorded is somewhat soot-stained, but is clearly of the west-Mexican race which differs from the Central American in having paler, less intensely yellow underparts and slightly larger bill. Except for the darker tinge caused by soot-stain, it is very similar to two birds from Escuinapa, Sinaloa. It may be pointed out that Hellmayr (Birds of the Americas, 1927, p. 109) has shown that *Laphyctes satrapa* Cabanis and Heine is a synonym of *Tyrannus couchii* Baird and that the proper name of the race ranging from southern Mexico south through Central America is *Tyrannus melancholicus chloronotus* Berlepsch.

In view of the subspecific status of the Washington bird, it would appear that a re-examination of Mr. Kermode's specimen is desirable. Logically, it should be of the north-west Mexican race rather than the Central American race.—A. J. VAN ROSSEM, *California Institute of Technology, Pasadena, California, May 25, 1929.*

EDITORIAL NOTES AND NEWS

We are editorially happy to say that *three* different Cooper Club members have now offered their services in compiling the third ten-year index to *The Condor*, namely, Mr. Milton P. Skinner, Mr. Griffing Bancroft and Mr. George Willett. The generosity of each of these persons, in offering to commit himself to this task, can only be understood by one who has attempted similar undertakings and thus knows the amount of painstaking labor involved. Upon Mr. Willett has been conferred final choice, because of his central location and the library and clerical facilities close at his hand. He is beginning the work at once, and there is fair chance that the index will be ready for publication about the middle of 1930.

This note is to remind Cooper Club members that the award of the Mailliard Prize in Western Ornithology (one hundred dollars) is due to be made in January, 1930. As set forth in *The Condor* of last January (XXXI, pp. 40-41), this prize is open competitively to any member who submits written report, explicit though not necessarily technical in language, upon some phase of bird study carried on in western North America. It is the intention of the donor of this prize to encourage persons not located in actively scientific centers to make independent, intensive studies of living birds. Full details concerning this competition can be had through enquiry of the Editors of *The Condor*.

Old-time Cooper Club members will find much entertainment in reading Harry S. Swarth's History of the Club, an abundantly illustrated, neatly printed brochure of 80 pages. This was distributed, as a special feature of the evening's program, at the Fourth Annual Dinner of the Club in San Francisco. It is an accurate record of our early personnel and its activities, put together in attractive literary form in which a vein of humor frequently becomes evident. While this publication was privately printed for distribution on the occasion mentioned, we understand there are copies still to be had by addressing either its author or Mr. W. Lee Chambers.

In Colorado, ornithological activities now center in the Colorado Museum of Natural History, at Denver, and in the University of Colorado, at Boulder. The State Museum, for many years maintained in Denver, has discontinued its natural history work and has distributed a large number of its ornithological specimens to the Colorado Museum of Natural History and a share of the mounted specimens to the University. At the latter place, where the Bergtold and Freeman collections totaling 1500 specimens are housed, new specimens are being added at a rapid rate. Courses in ornithology are taught during the spring and summer quarters, though but little research is undertaken. Items of interest are occasionally published. A recent major contribution of marked excellence was Junius Henderson's "Practical Value of Birds." The Colorado Museum of Natural History has assembled for the benefit of teachers, students and others interested, a practically complete mounted collection of the birds of Colorado, accompanied by selections of the birds' eggs. Some habitat groups of Colorado birds are on display and groups of South American birds are being added. Mr. Robert J. Niedrach, Ornithologist at the Museum, aids with the bird work in the schools and also directs Boy Scout nature work. The Director of the Museum, Mr. J. D. Figgins, has recently returned from South America, where he was collecting materials for exhibition purposes.

Dr. Erwin Stresemann, of the Zoological Museum of the University of Berlin, sends us the following information, of importance especially to bird banders. "*Chaetura vauxi* is wintering every year in great numbers in an old chimney of the coffee plantation El Zapote, near the Volcano Fuego (about lat. 14° 34′ N, long. 90° 31′ W), Guatemala. They arrive in December and begin to leave the spot in

March. At the end of March only a small number is left. They are molting during February and March and are not migrating back before the molt is nearly completed. Mr. Walter Wenzel, who drew my attention to this fact and published a note about it in the Ornithologische Monatsberichte (XXXVI, 1928, p. 76), has now started to mark the birds in the chimney with rings of the Biological Station, Helgoland (Germany). He ringed 99 of them on April 7, 1929, and is prepared to continue the scheme in 1930 on a larger scale. The guess, that the species is *Chaetura pelagica*, has proved to be an error. I since got a specimen for identification and found it to belong to *Chaetura vauxi*. It may be advisable to draw the attention of California ornithologists to this fact. A note about recovered birds would be welcome to Dr. R. Drost, Biologische Anstalt, Helgoland, Germany."

An item in "The official Record," United States Department of Agriculture (volume 8, number 20, May 16, 1929) records the shipment of some 3,000 live Mexican quail to Bologna, Italy, for "re-stocking purposes". These quail were brought into the United States from Mexico at Brownsville, Texas, and were shipped through an American importer on February 28 of this year. We can thus expect practically no limit in the transportation of game species from one place to another all over the world! It is curious to us that Government authorities should not only approve but apparently encourage such activities which are, admittedly by most serious students, of dubious propriety. In this connection, we see by the daily press that one Carl Ring, of the San Diego Zoo, is ardently advocating the stocking of southern California with various kinds of foreign pigeons and doves, so that our depleting "bird life would gain immensely". It is implied that our native Mourning Dove would in no wise suffer; there is "room for all". Fortunately San Diego has citizens of a sounder point of view. We understand that the San Diego Society of Natural History, through its Director, Mr. Clinton G. Abbott, has announced itself as opposing any such liberations of non-native birds. Mr. Frank F. Gander, another bird student of San Diego, asks cogently, "Why not practice *conservation* of what we have rather than *replacement?*"

MINUTES OF COOPER CLUB MEETINGS

NORTHERN DIVISION

APRIL.—The regular monthly meeting of the Northern Division of the Cooper Ornithological Club was held on April 25, 1929, at 8:00 p. m., in Room 101, Zoology Building, University of California, Berkeley, with about one hundred members and guests in attendance. Upon the invitation of Vice-president Clabaugh, Mr. Joseph Mailliard occupied the chair. Minutes of the Northern Division for March were read and approved. Proposals of membership made at the March meeting of the Southern Division were read. New names proposed for membership were: Mrs. Helena E. Lindsey, Route 4, Box 30, Hayward, Calif., by Mrs. G. Earle Kelly; Mr. W. R. Penny, Hotel Carlton, Berkeley, Calif., by Genevieve S. Burk.

A letter of appreciation from Mr. J. Eugene Law was read, in which he thanked the Northern Division for its part in electing him to Honorary Membership in the Club. A motion was made by Mr. Clabaugh that since the Annual Meeting of the Cooper Ornithological Club is to be held in the Bay region in May, the usual monthly meeting of the Northern Division be omitted. This motion was seconded and unanimously carried.

Dr. Ritter requested help in replying to a correspondent who wished to know of some way in which he might divert the attacks of a Red-shafted Flicker upon the gable-end of his house. Mr. Clabaugh reported the finding of 26 occupied birds' nests among trees bordering a Fresno orchard on April 21. Ten of these nests proved to belong to mockingbirds. Mr. Mailliard reported the gratifying fact that California Clapper Rails are now to be seen along the slough near Mill Valley Junction in Marin County, and Dr. Bryant added that these birds have been noted also near Baltimore Park, a fact as gratifying as the increase, under protection, of Cranes and Band-tailed Pigeons.

Mrs. G. Earle Kelly gave the evening's talk, upon a "Trip to the Tropics in Winter". Leaving San Francisco on December 8 by steamer, Mrs. Kelly had the pleasure of studying birds at Fort Randolph, Panama, Cristobal, and the Barro Colorado Island Laboratory on Gatun Lake in the Canal Zone, in Jamaica,

in Cuba, and on the Florida Keys. Mrs. Kelly's sprightly and vivid account of her journey and of the birds she saw, as well as of the natives' study of *her*, was most entertaining.

Adjourned.—HILDA W. GRINNELL, *Secretary.*

SOUTHERN DIVISION

MARCH.—The March meeting of the Southern Division of the Cooper Ornithological Club was held at the Arroyo Seco Branch Library, Pasadena and Piedmont avenues, Los Angeles, on March 26, 1929. President Harris called the meeting to order at 8 p. m., with about thirty members and friends present. The minutes of the previous meeting of the Southern Division were read and approved.

The following applications for membership were read: Jacob Bates Abbott, 3491 Country Club Drive, Altadena, Calif., Dr. John B. May, 136 State House, Boston, Mass., and Charles Eliot Underdown, Academy of Natural Sciences, Logan Circle, Philadelphia, Pa., all proposed by W. Lee Chambers; Elizabeth Shirley Jenkins, 3769 Grim St., San Diego, Calif., and Guy Edward Boothby, 904 Pomona, Coronado, Calif., both proposed by Frank F. Gander; and Albert E. Hodgkins, 347 East Flora St., Stockton, Calif., proposed by W. B. Sampson.

The following proposal of Mr. J. Eugene Law for Honorary Membership was read [see preceding Minutes, p. 139]. This proposal was signed by Loye Miller, W. Lee Chambers, Howard Robertson, G. Willett, and Wright M. Pierce. Upon the motion of Mr. Pemberton, which was duly seconded, this proposal was unanimously adopted.

President Harris announced that there is on exhibition at the present time at the Los Angeles Museum, Exposition Park, a collection of the bird paintings of Lynn Bogue Hunt. The sudden death of Mr. Robert Ridgway on the night of March 25 was announced by the President, who appointed Mr. Willett and Dr. Bishop a committee on resolutions.

Mr. Raymond B. Cowles, the speaker of the evening, told interestingly of some of the birds of South Africa. He first likened South Africa to southern California in climate, and stated that the topography of large areas is much like that in the vicinity of Puente, California, namely, small valleys and rolling hills. The large game is mostly destroyed except in reservations, and the bird fauna, though very rich, is rapidly being depleted. The Zulus kill the birds with powerful air rifles. Mr. Cowles told particularly of the characteristics and habits of the Umbrette, similar to our Heron, and the Hornbill, showing some photographs of each.

Adjourned.—HAROLD MICHENER, *Secretary.*

APRIL.—The April meeting of the Southern Division of the Cooper Ornithological Club was held at the Los Angeles Museum, Exposition Park, Los Angeles, on April 23, 1929, with President Harris presiding and about eighty members and friends present. The minutes of the March meeting of the Southern Division were read and approved. The minutes of the February and March meetings of the Northern Division were read.

The following applications for membership were read: Mrs. Edward L. Parsons, 2504 Pacific Ave., San Francisco, Calif., proposed by W. Lee Chambers; Mr. Alfred D. Trempe, 612 Kimball St., Sault Ste. Marie, Mich., and Mr. Jim Kitchin, Lake Henshaw, Santa Ysabel, San Diego County, Calif., both proposed by Harold Michener.

The committee on resolutions appointed at the previous meeting presented the following resolution which was unanimously adopted.

Whereas, in the death of Robert Ridgway at Olney, Illinois, on March 25, 1929, North American Ornithology has lost its foremost exponent, and the Cooper Ornithological Club of California an Honorary Member and an earnest, faithful friend, and

Whereas, from the time when a boy not yet seventeen he, as Zoologist, carried on the field work of the Geological Survey of the Fortieth Parallel for the United States Government, two years later preparing the scientific report on the birds collected, until the present, when at the age of seventy-eight he has passed away, his monumental work on the Birds of North and Middle America yet unfinished, a steady stream of papers and books on scientific ornithology of the highest order of excellence has come from his pen, and

Whereas, in botany and pictorial illustration he gave the world much of value and produced in his "Color Standards and Color Nomenclature" a book of much originality and one indispensable to all scientists in whose studies colors enter, and

Whereas, through his fifty years as Ornithologist of the United States National Museum, he not only kept the scientific work there on an elevated plane but gave freely of his time and learning to all that asked, therefore be it

Resolved, that we, the Southern Division of the Cooper Ornithological Club, desire to express our great thankfulness for his modest and unselfish work, his ability and his charming character, his unswerving uprightness and the knowledge which he showered on us all; our sincere sorrow that his useful life has ended; and to extend to his remaining family our heartfelt sympathy in their and our great loss; and be it further

Resolved, that these resolutions be spread upon the minutes of the Southern Division of the Cooper

Ornithological Club and that a copy be transmitted to his surviving brother and three sisters.

A letter from Mr. J. Eugene Law was read expressing his appreciation of the Club's action in electing him to Honorary Membership. A letter from Dr. Grinnell was read presenting the following resolutions which had been prepared by Mr. John G. Tyler, president of the Northern Division, and unanimously adopted by the Northern Division at its last meeting. [See Minutes, p. 139.]

Dr. Bishop moved the adoption of these resolutions. Dr. Miller suggested that it would be better for the Southern Division to indorse the action of the Northern Division in adopting the resolutions and to offer assistance to the committee appointed by the Northern Division rather than to appoint a separate committee; his thought being that the responsibility for the work should not be divided between two committees. Dr. Bishop explained that he believed the influence toward the end found desirable by the Cooper Club would be greater if presented as the findings of a committee from the north and one from the south, rather than as the findings of only a committee from the north. Mr. Howard Robertson in seconding the motion for adoption agreed with Dr. Bishop's thought on this point. The motion was carried. The President appointed Mr. van Rossem and Mr. Willett, chairman, on the committee and asked those two to choose a third member.

Mr. A. C. Bent was the speaker of the evening and he told in a most interesting way of a collecting trip taken in 1911 to the Aleutian Islands, Bering Sea and Northern Alaska. The main object of the expedition was to get a complete series of Ptarmigan. In this he was quite successful.

Adjourned.—HAROLD MICHENER, *Secretary.*

MAY.—The May meeting of the Southern Division of the Cooper Ornithological Club was called to order at 8 p.m. of the 28th by Vice-president Willett in the Los Angeles Museum, Exposition Park, Los Angeles. About forty-five members and friends were present. The minutes of both divisions for the April meetings were read by title only.

The following applications for membership were read: Robert Keech Gilbert, 101 No. Arden Blvd., Los Angeles, E. J. Thomas, 230 West 23rd Street, Los Angeles, and Dwight G. Vedder, 408 Quimby Bldg., Los Angeles, all proposed by Alfred D. Trempe; and Mrs. Benj. Little Clary, Coral Reef Ranch, Coachella, Calif., proposed by Harold Michener.

Dr. Loye Miller was called upon to tell of the Annual Meeting held May 17, 18, and 19 in Berkeley and San Francisco. He reviewed the history of the Annual Meetings of the Club, pointing out that the attendance and interest has increased with each succeeding meeting and that the one this year, the fourth, gives good reason to believe that the Annual Meeting has become an established part of our activities. He told of the various sessions for the presentation and discussion of papers and of the entertainment features provided by our northern associates. The Board of Governors meeting on the morning of the 19th was dwelt upon with particular reference to the work that has been done looking toward incorporation. Dr. Miller explained the benefits to be derived from incorporation and requested careful consideration and favorable action from the members when the question of incorporation is submitted to them.

The speaker of the evening, Mr. A. L. Pickens, was then introduced. He spoke most interestingly on "Hummingbirds in relation to form and color in flowers", citing and illustrating by means of crayon drawings many flowers which have developed into forms adapted for the utilization in one particular area or another of Hummingbirds to carry their pollen from blossom to blossom. The discussion which followed showed that Mr. Pickens had struck a responsive chord in his audience and he left with them an appeal for careful observation of birds, since birds may assist in the pollination of flowers. He asked that such observations be reported at Cooper Club meetings.

Adjourned.—HAROLD MICHENER, *Secretary.*

FOURTH ANNUAL MEETING

The Fourth Annual Meeting of the Cooper Ornithological Club was held in the San Francisco Bay region May 17 to 19, 1929. The program opened with a session at the California Academy of Sciences in San Francisco on the morning of May 17. Following a brief address of welcome by Dr. Barton W. Evermann, Director of the Academy's Museum, a response was made by President Loye H. Miller of the Board of Governors of the Club.

The scientific session began with a

paper on "Aëronautics in Bird Flight," by Sterling Bunnell, in which comparison was made between the manipulations used by men in the handling of aeroplanes in flight and the adjustments made by birds under corresponding conditions. "The Role of the Runt: A Taxonomic Problem," by J. Eugene Law, was an effort to point out that the species was best represented not by its average individuals but by the largest or brightest or most outstanding representatives. In "Notes on Wild Geese from Central California," James Moffitt detailed important features relating to the several species of geese which winter in the state, as reflected in recent observations.

Following the program of the morning, box lunches were served to the assembly through the courtesy of the Academy. The members then had opportunity to examine the exhibits in the Academy's Museum and the Steinhart Aquarium. The afternoon session opened with a discussion of "Faunal Conditions of Salvador," by A. J. van Rossem, based upon recent field experiences in that republic. H. S. Swarth showed that "The Eggs of Certain Galapagos 'Finches' and 'Creepers'" bear out his recently published opinion that the members of these groups represent a single distinctive family of birds. The "Growth and Reactions of the Barn Owl" were discussed by Gayle B. Pickwell. "Some Returns of Banded Birds" were discussed by John McB. Robertson who presented charts showing recent results in this field. The charts indicated the wide dispersal of some, even "resident" species after nesting. A notable feature was the number of returns from gulls, indicative of the large numbers of these birds shot in various parts of the west, despite the present laws protecting them. H. C. Bryant presented motion pictures taken by E. S. Cheney showing "Wintering Cranes in California." Joseph Mailliard offered "Gleanings from some Recent Bird Banding," based on observations in Golden Gate Park adjacent to the Academy and in Marin County. Small rather fixed local winter ranges for many individual Zonotrichias, as previously reported by other observers, featured these results.

The Annual Dinner was held at the Bellevue Hotel in San Francisco, with about ninety-five persons present. L. H. Miller acted as toastmaster and various persons responded briefly to his delightful introductions. A surprise feature was the distribution of the history of the Club from its earliest beginnings down to date, compiled with much painstaking effort by H. S. Swarth and published through the courtesy of an unknown donor to the Club. The members and guests were then entertained by a showing of slides and motion pictures of Laysan Island bird life, featuring slides taken by W. K. Fisher in 1903 and slides and films taken in 1923 by D. R. Dickey, with running comments by the latter. The extermination of one or more species and great reduction in numbers of others, resulting from the introduction of rabbits on the island with consequent destruction of vegetation and subsequent damage to the terrain by tropical wind storms, was brought out forcibly, and demonstrated the danger incident upon man's introduction of alien species and disturbance of the natural balance.

The session of May 18 opened in room 113 Haviland Hall, University of California, Berkeley, with a discussion by Joseph Dixon of "California Gulls Nesting at Mono Lake," with illustrations of conditions in 1916. J. E. Law showed conclusively that "The Function of the Oil-gland" was not to provide water proofing for feathers. Feathers are not oiled and the gland has some other function, possibly maintenance of the beak in satisfactory condition. W. H. Burt discussed the "Morphology of the Woodpeckers with Reference to the Adaptations," dealing particularly with the Red-shafted Flicker, California Woodpecker, and Hairy Woodpecker, and showing the progressive specialization exhibited by these forms. "A New Miocene Cormorant from Calabasas" was exhibited in picture form and briefly discussed by L. H. Miller who also presented briefly a communication from Hildegarde Howard on Neogyps. Miss Leigh M. Larson discussed "The Osteology of the Roadrunner, Recent and Pleistocene", and showed that no significant difference could be detected between the birds of the present and those of the Pleistocene as indicated by material from Rancho La Brea. "The Fossil Passerines of Rancho La Brea" were discussed by Alden H. Miller.

Lunch for the Club was provided at Drake's Restaurant and during the balance of the noon recess opportunity was afforded those in attendance to view an

exhibit of bird photographs in the exhibition room of Haviland Hall, featuring pictures by Joseph Dixon, W. M. Pierce, E. L. Sumner, Jr., and J. A. Calder. The afternoon session opened with a discussion of the "Distribution of the Wedge-tailed Shearwater," by Mrs. M. E. McLellan Davidson. Bert Harwell presented a brief progress report on "The Campaign to Elect a State Bird for California" and indicated that the Valley Quail had received by far the largest public preference so far. W. E. Ritter, under the title "A Virgin Field for a Revised Type of Research in Zoology," called attention to the splendid opportunities available for study of bird activities. The "Status of Some Pacific Coast Clapper Rails" was discussed by A. J. van Rossem. Joseph Grinnell called attention to "Some Angles of the General Problem of Bird Migration," and emphasized the fact that migration is after all a type of adjustment to meet immediate conditions and therefore subject to change at any slight stimulation. As indicative of the new observations often afforded in a well worked area, A. H. Miller described a local nesting colony of "The Black-chinned Sparrow in the San Francisco Bay District," which appeared recently in the hills east of Berkeley. Under the caption "Roadways: As they Affect Bird-life," Jean M. Linsdale indicated that roadways must be looked upon as a type of habitat to be reckoned with in any plan discussing the local occurrence of various species of birds. "The Economic Status of Some California Birds" was discussed by D. D. McLean with especial reference to recent campaigns upon the part of insufficiently informed individuals in the San Joaquin Valley who are attempting wholesale clean-ups of the bird inhabitants of orchards and vineyards.

Repeating the pleasant experiences of the second annual meeting, George M. Wright acted as host in the evening and opened his home to upwards of 110 members and guests. A delightful supper was followed by a brief program featuring impressions of bird life of the Panama Canal Zone by Mrs. Junea W. Kelly and an informal analysis of bird voices and notes by L. H. Miller.

Approximately 65 persons were in attendance at each one of the scientific sessions and a total of more than 100 individuals were represented in the four half-day meetings. Of this number, 20 or more had come from south of Tehachapi. There is a general feeling that the four annual meetings thus far held have demonstrated beyond a doubt the desirability of this sort of opportunity for western students of ornithology to join in discussing their particular problems; it is believed that the annual meetings will continue henceforth on a well established basis.

GOVERNORS' MEETING

The Eighth Annual Meeting of the Board of Governors of the Cooper Ornithological Club was held at the residence of Dr. and Mrs. J. T. Allen, 37 Mosswood Road, Berkeley, on the morning of May 19. Prior to the business session, Mrs. Allen and Mrs. H. W. Grinnell jointly served breakfast for the members of the Board.

Eighteen members were present in person and 7 were represented by proxy, from the total membership of 33. Minutes of the Seventh Annual Meeting were read and approved. The report of the business managers indicated that the Club was in good financial condition. An audit of the Club's accounts by W. J. Boland, public accountant, was provided through the courtesy of D. R. Dickey. The Board voted to affiliate the Cooper Ornithological Club with the Associated Sportsmen of California, designating Joseph Dixon as contact representative. The resignation of H. S. Swarth as associate editor of the Condor was accepted and J. M. Linsdale was selected to fill his place. D. R. Dickey presented a report of the Committee of Incorporation. The name of the corporate body when formed will be the Cooper Club, Inc., and its governing unit will be a Board of Directors. The President and Secretary of the present Board of Governors together with Howard Robertson as counsel were authorized to visé the proposed articles of incorporation and the proposed new constitution for the Cooper Ornithological Club (not incorporated) and proceed with incorporation. The need for incorporation arises out of the fact that the Club as now constituted can not receive bequests under state law. Much effort has been devoted to developing a scheme of incorporation which will continue the two Divisions and such chapters as are desirable from time to time, the corporation being merely designed to look after the property interests of the organization as a whole.

Adjournment was taken at 1:20 p. m.
—TRACY I. STORER, *Secretary.*